EXHIBIT 37

UNITED STATES DISTRICT COURT

NORTHERN DISTRICT OF ILLINOIS

EASTERN DIVISION

ANDREW CORZO, SIA HENRY, MICHAEL MAERLANDER, ALEXANDER LEO-GUERRA, BRANDON PIYEVSKY, BENJAMIN SHUMATE, BRITTANY TATIANA WEAVER, and CAMERON WILLIAMS, individually and on behalf of all others similarly situated,

Plaintiffs,

v.

BROWN UNIVERSITY, CALIFORNIA INSTITUTE OF TECHNOLOGY, UNIVERSITY OF CHICAGO, THE TRUSTEES OF COLUMBIA UNIVERSITY IN THE CITY OF NEW YORK, CORNELL UNIVERSITY, TRUSTEES OF DARTMOUTH COLLEGE, DUKE UNIVERSITY, EMORY UNIVERSITY, GEORGETOWN UNIVERSITY, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, NORTHWESTERN UNIVERSITY, UNIVERSITY OF NOTRE DAME DU LAC, THE TRUSTEES OF THE UNIVERSITY OF PENNSYLVANIA, WILLIAM MARSH RICE UNIVERSITY, VANDERBILT UNIVERSITY, and YALE UNIVERSITY,

Defendants.

Case No. 1:22-cv-00125

Class Action

REBUTTAL EXPERT REPORT OF GEORGE BULMAN, PH.D.

October 7, 2024

This report cites and quotes material that Defendants designated as Confidential or AEO under the Second Amended Confidentiality Order (ECF No. 608).

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I. INTRODUCTION

1. Counsel for Plaintiffs have asked me to review the expert reports of Dr. Bridget Terry Long ("Long Rpt."),[1] Dr. David L. Yermack ("Yermack Rpt."),[2] Dr. Nicholas Hill ("Hill Rpt."),[3] and Dr. Peter Ammon[4] ("Ammon Rpt.") (collectively, "Defendants' Experts") to the extent they address my Report dated June 10, 2024.[5] Their criticisms of my Initial Report are all without merit and do not cause me to alter the following main conclusions therein: Based on a statistical analysis using the well-recognized method of regression analysis, Defendants spent less on institutional aid out of their "excess returns" on their endowment investments when they were participating in the Challenged Conduct**,** a result that is consistent with the Overarching Agreement and Defendants' participation in it; and all Defendants could have substantially increased their institutional aid during the Class Period while increasing the purchasing power of their endowments.

2. In this Rebuttal, I first summarize several broad critiques of my analysis that run through some or all of the Defendants' Experts' reports, and then discuss broadly why each of these critiques lacks merit. I then demonstrate, in sequence, why the critiques found in each of the Defendants' Experts' reports are mistaken.

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1. Expert Report of Bridget Terry Long, Ph.D. (August 7, 2024).
2. Report of David L. Yermack (August 7, 2024).
3. Expert Report of Nicholas Hill (August 7, 2024).
4 Expert Report of Peter Ammon (August 7, 2024).
5. Expert Report of George Bulman (May 14, 2024) ("Initial Report"). All initial capitalized terms in this Reply Report have the same meanings as used in my Initial Report.

II. SUMMARY OF MY RESPONSES TO DEFENDANTS' EXPERTS' CRITIQUES AND THE REASONS WHY THEY ARE INVALID

3. I summarize in this opening section the broad critiques of my main conclusions and the broad reasons why these critiques are misplaced. I set out in the next section my detailed responses and specific references, where I rebut the specific critiques aimed at my Initial Report.

4. Defendants' Experts do not appear to dispute the finding in my Initial Report that all Defendants experienced substantial investment returns and endowment growth before and during the Class Period. This is not surprising, because this is what the data clearly show. One critique asserts that I failed to account for graduate students in reporting endowment per student. Yermack Rpt. at p. 21. This is wrong, as I explain below.

5. Defendants' Experts make several critiques of my statistical analysis showing that Defendants spent less on institutional aid out of their "excess returns" on their endowment investments after the Challenged Conduct began. Long Rpt. at p. 212. Yermack Rpt. at p. 22. Hill Rpt. at p. 138. Ammon Rpt. at p. 6. Their critiques of my excess-returns variable and my measures of statistical significance (reflected in the "standard errors" that are applied to the estimate coefficients in the regressions) are wrong. Defendants' Experts' assertions that the Financial Crisis, and not Defendants' participation in the Challenged Conduct, caused the changes in their spending of institutional aid out of excess returns are not supported by the evidence. Defendants' experts claim that Defendants in general, and Penn in particular, have been especially generous with their endowment spending. Long Rpt. at p. 222. Ammon Rpt. at p. 4. But these experts are wrong, and they do not rebut my statistical test, which assesses whether Defendants *changed* their spending on institutional aid when they were participating in the Challenged Conduct relative to when they were not. Defendants' experts assert that my analysis

did not properly account for new gifts. Yermack Rpt. at p. 17. This is wrong. The claim that there is no relationship between endowment returns and endowment levels, Hill Rpt. at p. 138, is wrong as a matter of fact. Defendants' experts' assertion that my regression results show that institutions became more generous after the start of the Challenged Conduct, Hill Rpt. at p. 141, are shown to stem from a misinterpretation of fixed effects and their role in the regression analysis.

6. Several of Defendants' experts disputed my finding that Defendants could have substantially increased their institutional aid during the Class Period while increasing the purchasing power of their endowments. Long Rpt. at p. 207. Yermack at p. 13. Ammon Rpt. at p. 3. One of their lines of attack is that a large portion of Defendants' endowments consisted of restricted endowment funds, which allegedly would be unavailable for providing more institutional aid. As my Initial Report explicitly considered, a focus on restricted endowments is misplaced because a significant fraction of endowment assets is unrestricted, a significant fraction of restricted endowment assets is earmarked for student aid, and additional restricted assets are for targeted to priorities that are likely to free up operating funding to be used for other purposes. All of these factors negate much of the effect of the restrictions. I also specifically rebut attacks on my finding in my Initial Report relating to endowments: that my analysis was retrospective rather than prospective, and that Defendants had ample endowments to support a 10% increase in institutional aid without jeopardizing the ability of those endowments to support future growth. Long Rpt. at p. 220. Yermack Rpt. at p. 35.

7. Finally, I rebut the specific data-related critiques regarding my use of NACUBO data on institutional spending rates, Yermack Rpt. at p. 23, and regarding the erroneous assertion that my

analysis of endowment per student failed to account for graduate students. Yermack Rpt. at p. 21.

III. RESPONSES TO SPECIFIC DEFENDANTS' EXPERT REPORTS

A. Dr. Long's Critiques Are Misplaced

8. Dr. Long's report advances several critiques of the conclusions in my Initial Report. None of them are valid. Dr. Long does not contradict my first conclusion, that Defendants had substantial growth in both investment returns and their endowments over the time periods covered by my initial report. Accordingly, I group and then rebut her specific critiques that appear aimed at the other two conclusions in my report.

1. Dr. Long's Misplaced Critiques of My Statistical Analysis

9. Dr. Long advances various critiques with the apparent aim of discrediting my statistical finding that Defendants spent less on institutional aid out of their "excess returns" on their endowment investments after the Challenged Conduct began.

10. First, Dr. Long argues that the Defendant institutions became increasingly generous over time with their undergraduate students. Long Rpt. at p. 35. In Figure 3 she presents the amount of institutional aid provided to students at the Defendant institutions from 2009 to 2022 as evidence of these institutions' increasing generosity. The apparent implication of Dr. Long's generosity claims is that they somehow defeat my statistical analysis.

11. As a matter of fact, Dr. Long's argument and Figure 3 ignore the list price that institutions charge for tuition and room and board.[6] Given that institutional aid is a discount off

[6] Figure 3 in Dr. Long's report also ignores enrollment growth and does not represent per-student institutional aid generosity. Had she done so, it would have been apparent that per student institutional aid grew less rapidly than the absolute amount of such aid.

tuition, the net education-related revenue these institutions collected is therefore calculated as total revenue from list prices minus all institutional aid. Table 1 below presents the inflation-adjusted net revenues, for all 17 Defendant universities combined, for the first-time full-time students considered in Dr. Long's report. In the Table, I include all the years in which at least some members of the 568 Group were active, beginning with the 2003-2004 cohort and concluding with the 2021-2022 cohort**.** The middle column of the table for the years 2009-2022 matches Figure 3 of Dr. Long's report (that is, $472 million of institutional aid in 2009). Table 1 reveals that, in real terms (after adjusting for inflation using the CPI), the growth in total list cost charged to students significantly outpaced the growth in total discounts (or institutional aid). In particular, the inflation-adjusted, real change in revenue collected from students, after discounts, for all 17 Defendant universities combined, over the 2004-22 period, was large and positive. This result is not consistent with the assertion that the Defendant institutions became increasingly generous over time. Moreover, Long's claim does not take account of the much more rapid growth of endowments of the 17 Defendant universities than their revenues collected from students. The last column of Table 1 presents the total real endowment total for all 17 Defendant universities over the full 2004-22 period. It shows that real, inflation-adjusted student revenue grew by 33% over a period during which real endowment values grew by 128%.

Table 1: Total Revenue From Full-Time, First-Time Students (in millions of dollars)

	First-Time, Full-Time Students			
Year	List Cost	Inst. Aid	Revenue	Endowment
2004	1,445	341	1,104	96,510
2005	1,475	342	1,133	105,814
2006	1,528	365	1,163	120,292
2007	1,588	382	1,206	144,414
2008	1,602	406	1,196	140,574
2009	1,712	472	1,240	107,675
2010	1,779	545	1,235	113,581
2011	1,810	560	1,250	128,994
2012	1,860	579	1,281	126,360
2013	1,906	594	1,312	134,649
2014	1,951	594	1,358	153,526
2015	1,978	601	1,377	160,166
2016	2,026	641	1,385	153,548
2017	2,082	661	1,421	166,561
2018	2,149	716	1,433	177,472
2019	2,197	741	1,456	184,817
2020	2,229	755	1,474	187,075
2021	2,143	736	1,407	250,808
2022	2,234	771	1,463	220,162

Notes: List Cost is the sum of tuition and room and board charged to each first-time, full-time student. Institutional aid is the sum of institutional aid for each first-time, full-time student. Revenue is the difference between the List Cost and Institutional Aid. Endowment is the sum of the endowment values. The table includes each of the 17 Defendants and each variable is inflation adjusted to 2022 dollars using the CPI.

12. Second, Dr. Long argues that institutions became increasingly generous over time despite the volatility in endowment returns. See, for example, Long Rpt at p. 223 & Figure 13. This

argument does nothing to discredit my statistical analysis of how Defendants' spending on institutional aid changed when Defendants were participating in the Challenged Conduct.

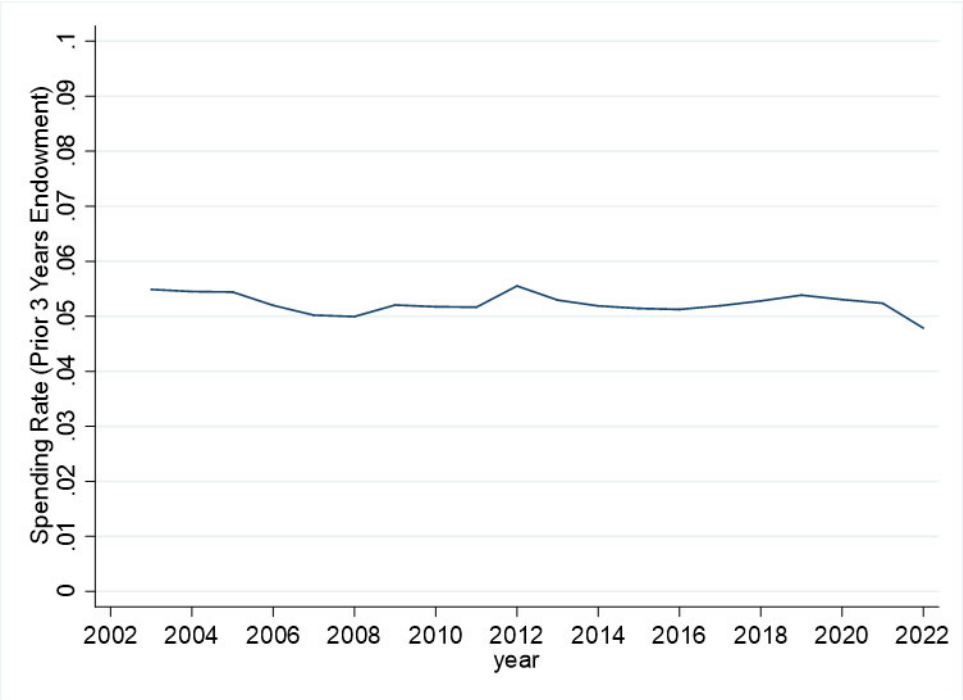
13. In addition, Dr. Long's "generosity analysis" considers only the period from 2009 to 2019, and thus does not capture the period of the Challenged Conduct or many of the years in which Defendants' endowments achieved their highest investment returns. Dr. Long's Figure 13 graph conveys that institutions use endowment-spending rules to smooth distributions over time, which is well known; it does not provide evidence that institutions became more generous with their endowments over time, nor does it convey the scale of endowment returns and growth relative to endowment spending over the period of the Challenged Conduct, from 2003 to 2022.[7]

14. To correct for the foregoing shortcomings in Dr. Long's generosity analysis, my Figure 1 presents the lagged spending rate for the Defendant institutions during the period of the Challenged Conduct. That is, it divides current-year endowment spending by the average of the prior three years endowment levels. Dividing current endowment spending by prior-year lagged endowment values captures the common practice by institutions of determining endowment distributions by applying a fixed payout percentage to the average endowment over the prior three years.[8] This figure shows that Defendant institutions did not increase their spending rates as their endowments grew tremendously in real terms over time, contrary to the pattern depicted in Long's Figure 13.

[7] Note that Figure 13 in Dr. Long's report presents "median" endowment distributions and returns, which should not be confused with average changes or returns and will not reflect institutions with the largest changes in spending or the highest returns.

[8] My Initial Report includes a more detailed discussion of endowment spending rules and their reliance on lagged endowment values. Initial Report at p. 19.

Figure 1: Lagged Endowment Spending Rate Over Time



Notes: This table presents the average spending rate from the endowment for the Defendants. The spending rate in the current year is divided by the average endowment level at the beginning of the prior three years. This calculation reflects the spending rules used by many institutions, which allocate endowment funds as a percentage of lagged endowment values. Northwestern's spending rates were not reported consistently during this period and thus are excluded.

15. In order to provide a sense of scale, Figure 2 presents endowment levels alongside annual endowment distributions for the period of Challenged Conduct. Figure 3 presents how cumulative endowment returns during this period compare to cumulative endowment spending.[9] These graphs reveal that while endowment returns are more volatile than endowment spending, they also are much larger in magnitude, which is an important reason for the massive growth in

[9] Figure 3 does not capture further endowment growth due to the initial value of new gifts, and concentrates only on endowment growth due to investment returns.

Defendants' endowments during the period of Challenged Conduct. The endowment returns come after even faster endowment growth during the 1990s. *See* Initial Report at p. 11.

16. In short, the evidence reflected in Table 1 and Figures 2 and 3 reveals that Defendant institutions experienced significant real endowment growth during the Challenged Conduct period while simultaneously increasing the revenue they collected from students. This evidence is not consistent with growing "generosity."

Figure 2: Endowment Levels and Spending (in billions of 2022 dollars)

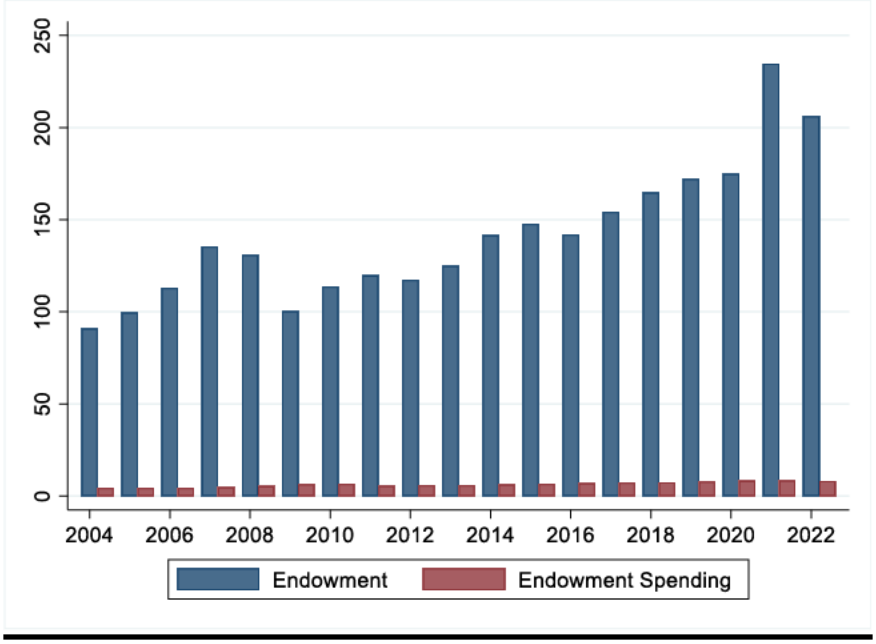
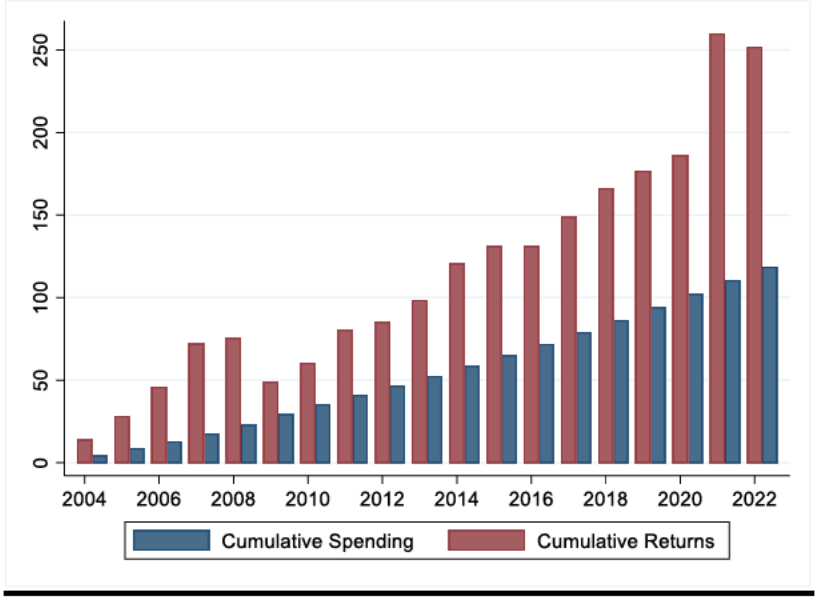


Figure 3: Cumulative Endowment Returns and Spending (in billions of 2022 dollars)



17. Third, Dr. Long's report includes a section titled "Volatile 'Excess Returns' Cannot Be Depended On for Decision-Making," and from this she argues that "Spending decisions, therefore, are not made based on reactions to so-called 'excess returns' in a particular year or even over a three-year period." Long Rpt. at p. 224. This argument is flawed because it confuses cumulative excess returns with short-run, volatile returns. In fact, she argues in the final five pages of her report that my report is invalid because it supposedly focuses only on short-run returns. Long Rpt. at p. 220-24. Dr. Long is wrong.

18. Contrary to Dr. Long, Rpt. at p. 220, the "excess returns" that I focused on in my Initial Report are not short-run returns and specifically are not just the returns in the prior three years. Instead, my analysis averages *long-run* cumulative returns to capture the fact that institutional endowment-spending rules are based on three years of lagged endowment values. Initial Report at p. 18 and p. 20. That is, to capture the mechanical relationship between long-run cumulative endowment returns and current spending accurately, I had to account for the fact that spending rules are not a function of the current endowment, but rather the endowment at the start of the

prior three years. Initial Report at p. 18. Dr. Long mischaracterized this careful adherence to the way in which endowment spending rules work as if I had relied on only short-run returns. In addition, my report describes in detail how my excess-returns measure relates to institutions' spending rules. I provided the code and data for these excess returns to the Defendants. The lagged excess-return measure that I used in in my Initial Report is specifically calculated to capture the smoothing that institutions use when spending from their endowments. Further, focusing on excess returns, above inflation and spending from their endowments, isolates the component of investment returns that leads to endowment growth and increased spending over time. Initial Report at p. 18.

19. Dr. Long's report also argues that my use of excess returns suffers from "hindsight bias." She writes: "In any given year, "excess returns" cannot be reliably factored into Defendants' contemporaneous annual budget projections for planning purposes because Defendants would not have the benefit of Dr. Bulman's and Dr. Singer's hindsight at the time they were making spending decisions. Short-run increases in returns are not sufficient to cover long-term spending commitments and administrators cannot assume past "excess returns" will be sustained in the future to cover new commitments." Long Rpt. at p. 213. She also asserts: "As described throughout this section, short-run 'excess returns' are not relied upon for planning purposes." *Id*. at 223.

20. Dr. Long's "hindsight" critique is based on a flawed understanding of my excess-return measure. My analysis captures the mechanical relationship between prior investment returns and endowment spending through institutions' own endowment-spending rules, which are based on prior year endowment levels. Institutions determine the contributions of their endowments to annual budgets based on the rolling average of prior-year endowment levels. These endowment

levels are in part a function of cumulative investment returns. My measure of lagged excess returns is constructed to capture this budgeting process based on lagged endowment values, and in no way assumes that short-run, current returns will be used to cover future spending. In short, my excess-returns measure is not based on short-run returns or "hindsight," as Dr. Long incorrectly asserts.

21. Dr. Long asserts that increasing aid generosity for current cohorts would jeopardize the Defendants' ability to provide aid to future cohorts. She states that "Increasing financial aid for one year as a result of so-called 'excess returns' would not be equitable to future students who by chance happen to be at college during a financial downturn where there are no so-called 'excess returns' available to provide the same level of institutional financial aid." Long Rpt. at p. 218. This assertion is wrong not only because it assumes that my analysis is based on short-run returns, but also because it assumes that additional spending by the Defendants would cause institutions to have too few resources to provide aid to future cohorts. My Initial Report finds that increasing aid by 10% in every year of the Challenged Conduct, a period during which endowment growth averaged 144%, would have reduced the endowment levels of the 17 Defendant universities by an average of just 3.9% in 2022. Initial Report at p. 31. Dr. Long thus is wrong in her assertion that the Defendants could not increase aid to current cohorts without jeopardizing their ability to provide aid to future cohorts. Further, a natural conclusion of her logic is that institutions should never spend their endowment returns, regardless of how massively endowments grow, because the future is hard to predict.

22. In addition, Dr. Long's critique does not capture the effects of endowment returns on institutional aid during the periods in which the Challenged Conduct took place. Dr. Long does not attempt to exclude institutions during periods before they entered into or after they stopped

participating in the Challenged Conduct. This mixing of conduct and non-conduct periods creates a disconnect between the analyses and the Challenged Conduct. For example, Figures 2, 3, 4, and 5 in her report depict spending, institutional aid generosity, and costs, and include Yale through the period from 2009-2022, when in fact Yale was not a member of the 568 Group from 2009 through 2017. The 2009-2017 period coincides with Yale having significantly increased the fraction of students who receive institutional aid and the amount of that aid relative to the average of the other Defendants.[10] My regression analyses, and my examination of excess returns and the impact of increasing aid, account for the years during which institutions claim not to have participated in the Challenged Conduct.

23. Dr. Long claims that data for institutional, federal, and state aid are not available in IPEDS prior to 2009. See, for example, Figures 3 and 4 in her report, in which she states that data on institutional, federal, and state aid for full-time, first-time students are not available prior to the 2008-2009 cohorts. In fact, data on institutional, federal, and state aid are available in IPEDS starting with the 1999-2000 cohort, and I used these data to examine aid and endowment returns in the period before and after the Challenged Conduct. I noted in my report that I used this data. Initial Report at p. 8. I also provided to the Defendants, after submitting my Initial Report, the data and code necessary to conduct the analysis. Dr. Long's rebuttals to my analysis based on this data are invalid because they do not include all of the data that I used and were available to her.

[10] Yale increased the fraction of first-time, full-time students receiving institutional aid from 45% to 56% between the 2007-2008 and 2008-2009 cohorts, and the average amount of institutional aid increased by approximately $5,100. In contrast, the other Defendants had 50% of students receiving aid in each of these two cohorts and average aid increased by $2,700 (unadjusted for inflation).

2. Dr. Long's Critiques Do Not Undermine My Showing that Defendants Could Prudently Have Increased Institutional Aid Spending

24. Dr. Long advances several arguments to rebut my finding that Defendants could prudently have increased funding out of their endowments by 10% while increasing the real value of their endowments. She argues that endowment funds are restricted and therefore could not be allocated to institutions' desired purposes. Long Rpt. at p. 207. She states: "Even when endowments are unrestricted, the practical reality is that unrestricted endowments are sometimes earmarked during annual budgetary processes to serve a certain purpose year after year." Long Rpt. at p. 207.

25. Dr. Long's arguments in these regards reflect several omissions, misunderstanding, and errors. Dr. Long does not document the extent to which institutional endowments are restricted or unrestricted, the share of endowments that are restricted specifically for aid, or the overlap (and thus fungibility) between restricted endowment funds dedicated to professorships and academic programs with general fund spending. I elaborate these and other critiques in the paragraphs that follow.

26. First, I report in Table 2 that across all 17 Defendant universities over the 2004-2022 period, on average 45% of assets were unrestricted and only 21% of assets were permanently restricted. As shown in the Table, endowments represent a significant fraction of assets. Defendants' financial reports in 2022 further reveal that 32% of endowment assets were unrestricted.[11] The 17 Defendant universities have had and have extensive unrestricted assets over which they have discretion to allocate distributions to the purposes they choose, including

[11] The classification of assets as restricted and unrestricted has been consistently reported in institutional financial reports throughout the period of interest, while the classification of endowments as restricted or unrestricted was not consistently reported until more recent years.

institutional aid to discount the sticker prices these universities charge students for tuition, room, board, and fees. Further, high endowment returns increase the value of these unrestricted funds and thus the amount of revenue available to be spent on institutional priorities, including institutional aid.

Table 2: Restricted and Unrestricted Assets

		Defendants' Assets (in Billions of dollars)				Classification of Assets (%)		
				Temporarily	Permanently		Temporarily	Permanently
Year	Endowment	Total	Unrestricted	Restricted	Restricted	Unrestricted	Restricted	Restricted
2004	62.3	81.2	49.0	15.5	16.8	60%	19%	21%
2005	70.6	90.9	55.0	17.9	18.0	61%	20%	20%
2006	82.9	103.7	62.6	21.5	19.6	60%	21%	19%
2007	102.3	124.7	76.1	27.1	21.5	61%	22%	17%
2008	103.4	126.0	75.6	27.4	23.0	60%	22%	18%
2009	78.9	98.7	39.7	35.8	23.2	40%	36%	24%
2010	84.6	104.2	42.5	34.8	26.8	41%	33%	26%
2011	99.1	123.1	48.5	48.0	26.7	39%	39%	22%
2012	99.1	122.8	47.0	47.7	28.1	38%	39%	23%
2013	107.2	135.9	54.4	51.5	29.9	40%	38%	22%
2014	124.2	154.5	61.6	60.9	32.0	40%	39%	21%
2015	129.7	160.2	62.9	63.5	33.8	39%	40%	21%
2016	125.9	156.7	60.6	60.0	36.1	39%	38%	23%
2017	139.5	173.2	68.5	65.6	39.1	40%	38%	23%
2018	152.3	190.5	77.1	71.6	41.8	40%	38%	22%
2019	161.5	198.2	77.6	74.2	46.3	39%	37%	23%
2020	165.4	201.0	75.7	77.0	48.2	38%	38%	24%
2021	232.2	279.5	109.3	118.1	52.1	39%	42%	19%
2022	220.2	271.5	109.5	107.2	54.8	40%	39%	20%
Average	123.2	152.4	66.0	54.0	32.5	45%	34%	21%

Notes: Endowments and assets are aggregated for all 17 Defendants. They are presented in nominal dollars.

27. Second, Dr. Long's critique does not account for the fact that a significant fraction of *restricted* endowment funds *is in fact* designated for financial aid and scholarships. For example, in 2022 financial reports, Duke, Brown, Notre Dame, and Vanderbilt report that 30%, 32%, 37%, and 39%, respectively, of restricted endowment funds were designated for student aid and

scholarships. Because significant shares of restricted funds are dedicated to aid, it is incorrect to assume that endowment restrictions will create a disconnect between endowment growth and an institution's ability to spend on financial aid. Indeed, high investment returns will increase the value of these funds and the amount of endowment revenue available for aid. Investment returns should lead to net increases in aid unless endowment revenue is offset by reductions from other revenue sources. As noted by Avery, Ehrenberg, Hill, and Webber (2024), "endowment spending nominally directed toward financial aid may not functionally be spent on that category if money from a different institutional source is reduced."

28. Third, Dr. Long is also wrong because a significant fraction of restricted gifts dedicated to core priorities is fungible with revenue from other sources of funds. That is, having restricted endowment funds that pay for professorships and other academic needs frees up operating funds that would otherwise pay for these purposes, so that the universities can allocate these funds to financial aid, thereby reducing their net COA. Beyond restricted funds dedicated to financial aid, two of the largest categories of restricted endowment funds include chaired professorships and support for academic programs and divisions.[12] For example, Duke reports that 57% of its restricted endowments are designated for instruction, while Notre Dame reports that 52% of its restricted endowments are designated for academic and student programs and faculty chairs (Duke University Financial Statements 2022/2023; University of Notre Dame, 2022 Annual Report). High endowment returns increase the value of these funds, increase the revenue they generate, and free up general fund revenue for other purposes such as financial aid. While some restricted funds may be tangential to an institutions' priorities, and thus do not free up other

[12] Dr. Long reports that the most common restricted endowment gifts are dedicated to financial aid, academic divisions, and professorships and other staff positions. Long Rpt. at p. 208.

resources, the share of funds that are dedicated to academic support and faculty positions indicates a significant overlap with institutions' core missions. As noted in Ehrenberg (2009), institutions "encourage donors to make the allowable uses of their endowments as broad as possible."

29. Dr. Long is also wrong to ignore the fungibility of endowment revenue with revenue from other sources and the freedom this creates to increase institutional aid. This is a critical oversight. Several papers in the economics and legal literatures consider the fungibility of restricted endowment funds with other revenue sources. Conti-Brown (2011) concludes that many restrictions require institutions to spend endowment gifts on financial aid such that "it is difficult as an interpretive matter to deem these gifts really 'restrictive' to the universities." Page 723. The Congressional Research Service (2018) highlights that restricted endowments are "unlikely to impose a constraint . . . for the purpose of slowing tuition increases, because of the fungibility of money." Page 22. Ehrenberg (2009) notes that as long as the amount of the endowment earmarked for a specific purpose is less than the amount the university would otherwise allocate, then "the restriction on the use of the donor's endowment for financial aid is not an actual constraint on institution behavior." Page 2. Weisbrod and Asch (2010) argue that an "erroneous assumption is that gifts to endowment are typically restricted," noting the difference "between a fund balance being labeled "restricted" and the fund being truly restrictive," as a "gift that is apparently restricted may, and typically does, leave the nonprofit with wide discretion over expenditures." Page 46.

30. Fourth, Dr. Long's assertion that the 17 Defendant universities are limited in their ability to provide more generous financial aid (to provide larger discounts), Long Rpt. at p. 218, is inconsistent with the size of these universities' endowments, their unrestricted assets, and their

investment returns. My Table 3 below examines the share of endowment funds and unrestricted assets that would be needed to increase aid generosity by 10%. This exercise reveals that less than 0.4% of just unrestricted assets in any year would cover the cost of increasing aid generosity by 10%. This result highlights how even modest endowment returns above inflation could be used to substantially reduce the financial burden faced by students. The presence of restricted endowment assets does not preclude institutions from increasing aid generosity and reducing net prices.

Table 3: Cost to Increase Aid Relative to Endowment and Unrestricted Asset Levels

		Unrestricted		10% Inst. Aid as Percent	
Year	Endowment	Assets	10% Inst. Aid	Endowment	Unr. Assets
2004	62,294	48,991	92	0.15%	0.19%
2005	70,614	54,988	95	0.13%	0.17%
2006	82,865	62,609	105	0.13%	0.17%
2007	102,295	76,099	112	0.11%	0.15%
2008	103,419	75,613	125	0.12%	0.16%
2009	78,934	39,685	144	0.18%	0.36%
2010	84,629	42,505	170	0.20%	0.40%
2011	99,147	48,463	182	0.18%	0.37%
2012	99,132	46,976	191	0.19%	0.41%
2013	107,182	54,436	201	0.19%	0.37%
2014	124,191	61,624	205	0.16%	0.33%
2015	129,716	62,933	212	0.16%	0.34%
2016	125,925	60,629	227	0.18%	0.37%
2017	139,507	68,474	237	0.17%	0.35%
2018	152,276	77,065	261	0.17%	0.34%
2019	161,452	77,614	277	0.17%	0.36%
2020	165,441	75,739	291	0.18%	0.38%
2021	232,224	109,345	291	0.13%	0.27%
2022	220,162	109,531	322	0.15%	0.29%

Notes: Endowments, assets, and aid are aggregated for all 17 Defendants in each year. They are presented in nominal dollars.

B. Dr. Yermack's Critiques Are Misplaced

31. Dr. Yermack advances various critiques of my Report. All lack merit. I begin this section by responding to one factual inaccuracy: Yermack's claim that my report purportedly failed to account for graduate students when examining the wealth of institutions in terms of endowment per student. Yermack Rpt. at p. 3. Dr. Yermack is wrong. In addition to reporting endowment per undergraduate student, my Initial Report states: "Undergraduate enrollment grew by an average of 9.4% between 1991 and 2003 and by 19.9% between 2003 and 2022. Graduate student growth was somewhat higher between 2003 and 2022, leading to overall enrollment change of 37% between 2003 and 2022, due in large part to the expansion of master's programs, including online and hybrid programs. Real endowment growth per student, *including graduate enrollment*, averaged 83% between 2003 and 2022." Initial Report at p. 14 (emphasis added). I reported that even when graduate students are counted, endowment per student grew at the 17 Defendant universities grew substantially—a near doubling in real, per-student terms over the 2003-2022 period.

1. Yermack's Critiques of My Analysis Are Misplaced

32. First, Dr. Yermack claims that my estimates of endowment growth fail to account properly for new gifts. Yermack Rpt. at p. 17. Dr. Yermack's calculations overstate the relative importance of new gifts for endowment growth, however, by erroneously assuming that endowment distribution amounts are not a function of endowment levels. Yermack Rpt. at p. 18. To emphasize the importance of new endowment gifts over endowment investment returns, Dr. Yermack concludes that 41% of endowment growth between 2009 and 2022 resulted from new gifts while the remainder stemmed from investment returns. However, using Dr. Yermack's own numbers for this period, only 23.8% of endowment gains were from new gifts, while 76.2% of gains were from investment returns. The discrepancy between his calculation and mine exists

because Dr. Yermack artificially inflates the importance of new gifts by dividing gifts by the net increase in endowments after distributions to represent the share of endowment growth. This is a highly misleading way to capture the importance of new gifts and is based on a fundamentally flawed assumption about endowment distributions for several reasons.

33. For one thing, dividing gifts by the net change in endowment levels inherently overstates the impact of gifts on endowment growth. Using the same approach, one could state that investment returns account for 131% of endowment growth (endowment returns divided by the net change in endowment levels), a non-sensical result (because the sum of explanations for such growth, by definition, cannot exceed 100%).

34. In addition, Dr. Yermack's calculation erroneously excludes the fact that endowment distributions are a function of new gifts and endowment growth. That is, the endowment distributions are, through institutional spending rules, a direct function of the new gifts and prior endowment returns. Dr. Yermack assumes that, in the absence of gifts and returns, distributions would have been unchanged. That is, his calculation of endowment growth, and the share of this growth that can be attributed new gifts, assumes that the level of spending from the endowment did not increase as a result of new gifts. This oversight causes him to underestimate the amount of endowment growth that would have occurred in the absence of new gifts.

35. My Initial Report, in fact, specifically accounts for new gifts. Once new gifts become part of the endowment, they become part of the investment pool and contribute to future investment returns. My analysis includes all endowment investment returns, including those generated by new gifts. Dr. Yermack's documentation of the large magnitude of investment returns relative to the initial value of new gifts highlights the credibility of my focus on endowment returns as the primary driver of endowment growth in my analysis. Endowment returns differ substantially and

randomly across the Defendants, providing a credible way to estimate the effect of endowment returns on financial aid and effective institutional prices. Moreover, the fact that investment returns were so large underscores the importance of the finding in my Initial Report that, when they were participating in the Challenged Conduct, Defendants did not increase spending on aid or reduce net prices to the same degree as happened during years before the period of Challenged Conduct.

36. Second, Dr. Yermack advances a "placebo" test in an attempt to show that institutional aid did not respond to excess returns because of the Financial Crisis, and not because of the Challenged Conduct. Yermack Rpt. at p. 28. Yermack's "test" is both non-standard and invalid. Standard placebo tests examine if treatment effects are present when choosing a fake treatment period before or after the treatment (policy or event) of interest. In this case, Dr. Yermack attempts to conduct such a test using the onset of the Financial Crisis. However, his test is fundamentally flawed in two crucial ways.

37. One, Dr. Yermack does not restrict his attention to the post-conduct period (e.g., starting with the 2003-2004 cohort), and instead includes pre-conduct cohorts from 1999-2000 to 2002-2003. The correct way to implement the placebo test is to examine if returns affect aid in the years after the start of the Challenged Conduct and before the Financial Crisis (Athey and Imbens, 2017; Eggers, Tunon, and Dafoe, 2024; Huntington-Klein, 2021). Including the pre-conduct years in this analysis produces invalid placebo estimates in the pre-crisis period that reflect a mix of several pre-conduct cohorts and post-conduct cohorts.

38. Two, Dr. Yermack incorrectly identifies the cohorts that should be affected by the Financial Crisis. He treats the years 2006-2007 as the last unaffected cohort prior to the Financial Crisis. In practice, given the timing of aid offers and the timing of changes in endowment values

due to financial markets, the 2009-2010 cohort is the first whose initial institutional aid offers should have been affected by the Financial Crisis. Students in the 2007-2008 would have received their aid offers in the spring of 2007, before the Financial Crisis, and students in the 2008-2009 cohort would have received their aid offers months before the stock market crash of September 2008.

39. Using the correct range of years for the placebo test reveals that Dr. Yermack's critique is incorrect and that my initial analysis is very robust to the range of years considered. To examine whether aid is responsive to endowment returns after the onset of the Challenged Conduct and prior to impacts of the Financial Crisis, I implement the test correctly, starting with the first cohorts affected by the Challenged Conduct (generally 2003-2004 or 2004-2005 depending on the institution).

40. To shed light on which cohorts should be affected by the endowment shocks associated with the Financial Crisis, Table 4 presents the collective values of endowments of all Defendant universities at the start (July 1) of each fiscal year.[13] Note that July 1 is after students have received aid offers and accepted their admission offers. Dr. Yermack uses the 2006-2007 cohort as the last cohort to receive aid offer prior to the impacts of the Financial Crisis. This is incorrect, since neither this cohort, nor the 2007-2008 and 2008-2009 cohorts (whose offers would have been made in the spring of 2007 and 2008, respectively) could have been affected by the drop in endowment values associated with the Financial Crisis given the timing of that crisis. In fact, stock prices peaked in the fall of 2007 and crashed in late September 2008.[14] Students in the 2007-08 cohort would have received their financial aid offers by April 2007, months before

[13] Start of fiscal year endowment values for each of the Defendants are reported by NACUBO.
[14] *See*, e.g. Gerald P. Dwyer, "Notes from the Vault," Federal Reserve Bank of Atlanta, September 2009, https://www.atlantafed.org/cenfis/publications/notesfromthevault/0909.

stock prices reached their highest values that fall. Students in the 2008-2009 cohort could only have been affected if institutions anticipated future market fluctuations, though, as shown in Table 4, endowment values were as high in July 2008 as they had been in July 2007. In fairness, the 2009-2010 cohort was really the first cohort whose aid offers were made and accepted after financial markets had collapsed. Cohorts in years before then would not have been affected by the Financial Crisis, which is consistent with the timing of the collective endowment values of the Defendant universities shown in Table 4 below.

Table 4: Endowment Values at Beginning of Fiscal Year

Cohort	Endowment Value Beginning FY (July)
2002-2003	54,685
2003-2004	54,794
2004-2005	62,294
2005-2006	70,614
2006-2007	82,865
2007-2008	102,295
2008-2009	103,419
2009-2010	78,934
2010-2011	84,629
2011-2012	99,147
2012-2013	99,132
2013-2014	107,182
2014-2015	124,191
2015-2016	129,716
2016-2017	125,925
2017-2018	139,507
2018-2019	152,276
2019-2020	161,452
2020-2021	165,441
2021-2022	232,224
2022-2023	220,162

Notes: Endowments are aggregated for all 17 Defendants. They are presented in nominal dollars.

41. Table 5 below presents the results of regression equations estimating the effects of endowment returns on aid and net prices starting with the Challenged Conduct (2003-2004 or 2004-2005 depending on the institution) and concluding with each possible cohort thereafter. The estimates reveal no evidence that endowment returns lead to greater aid or reduced net prices or effective institutional prices for pre-crisis samples that include the 2007-2008 cohort or the 2008-2009 cohort. Indeed, the only estimates that suggest aid increased with endowment returns are found if one artificially, and improperly, truncates the sample with the 2006-2007 cohort.

Table 5: Regression Results Using Alternative Ending Cohorts

Sample (last cohort)	Inst. Grant Aid		Net Price		Effective Inst. Price	
2004-2005	-0.282	(0.555)	0.483	(0.724)	0.405	(0.576)
2005-2006	-0.022	(0.242)	-0.242	(0.288)	-0.214	(0.261)
2006-2007	0.014	(0.123)	-0.245*	(0.136)	-0.213**	(0.105)
2007-2008	-0.113	(0.102)	-0.009	(0.117)	-0.019	(0.090)
2008-2009	-0.120	(0.074)	-0.049	(0.088)	-0.002	(0.072)
2009-2010	-0.119*	(0.071)	-0.060	(0.084)	-0.006	(0.070)
2010-2011	-0.136*	(0.071)	-0.034	(0.082)	0.017	(0.070)
2011-2012	-0.147**	(0.071)	-0.001	(0.083)	0.041	(0.070)
2012-2013	-0.153**	(0.069)	0.039	(0.083)	0.071	(0.070)
2013-2014	-0.154**	(0.066)	0.074	(0.084)	0.095	(0.070)
2014-2015	-0.120*	(0.063)	0.052	(0.085)	0.064	(0.069)
2015-2016	-0.094	(0.058)	0.067	(0.074)	0.060	(0.060)
2016-2017	-0.067	(0.054)	0.076	(0.064)	0.051	(0.051)
2017-2018	-0.043	(0.049)	0.047	(0.065)	0.018	(0.053)
2018-2019	-0.026	(0.044)	-0.018	(0.070)	-0.038	(0.055)
2019-2020	-0.012	(0.040)	-0.060	(0.064)	-0.078	(0.051)
2020-2021	-0.046	(0.071)	0.008	(0.102)	-0.030	(0.083)
2021-2022	-0.044	(0.055)	-0.010	(0.083)	-0.038	(0.066)

Notes: This table presents egression estimates in natural logs for institutional grant aid, net price, and effective institutional price. Each regression includes Defendants beginning in their first year of Challenged Conduct and excludes years in which they did not engage in the conduct. Estimates are based on a specification that includes institution fixed effects and year fixed effects. Standard errors are Huber-White robust. The symbols *, **, and *** represent statistical significance at 10%, 5%, and 1%, respectively.

42. Indeed, if the Financial Crisis were driving the lack of institutional aid responsiveness to returns, one would expect institutional aid to become more responsive during the decade of high endowment returns after the Financial Crisis. Table 6 reports the results of a regression estimating the effect of endowment returns on aid and net prices for alternative starting years and going through 2022. These estimates reveal no significant effects even when starting in years long after the Financial Crisis. In sum, there is no statistical evidence that the Financial Crisis caused the lack of responsiveness of aid to endowment returns that I found in my Initial Report. Dr. Yermack's contrary conclusion only holds when improperly truncating the sample to exactly the 2006-2007 cohort.

Table 6: Regression Results Using Alternative Starting Cohorts

Sample (first cohort)	Inst. Grant Aid		Net Price		Effective Inst. Price	
2003-2004	-0.044	(0.055)	-0.010	(0.083)	-0.038	(0.066)
2004-2005	-0.026	(0.060)	-0.011	(0.090)	-0.043	(0.072)
2005-2006	-0.019	(0.067)	0.010	(0.100)	-0.026	(0.080)
2006-2007	-0.009	(0.076)	0.022	(0.118)	-0.024	(0.093)
2007-2008	0.017	(0.085)	0.012	(0.134)	-0.035	(0.106)
2008-2009	0.023	(0.090)	0.025	(0.144)	-0.029	(0.113)
2009-2010	0.030	(0.091)	0.002	(0.148)	-0.051	(0.115)
2010-2011	0.039	(0.092)	-0.024	(0.153)	-0.072	(0.119)
2011-2012	0.027	(0.097)	-0.043	(0.163)	-0.072	(0.127)
2012-2013	0.006	(0.107)	-0.027	(0.185)	-0.056	(0.143)
2013-2014	-0.015	(0.120)	-0.035	(0.209)	-0.040	(0.162)
2014-2015	-0.050	(0.132)	-0.003	(0.238)	0.005	(0.180)
2015-2016	-0.049	(0.141)	-0.023	(0.261)	0.005	(0.196)

Notes: This table presents regression estimates in natural logs for institutional grant aid, net price, and effective institutional price. Each regression includes Defendants until their final year of Challenged Conduct and excludes years in which they did not engage in the conduct. Estimates are based on a specification that includes institution fixed effects and year fixed effects. Standard errors are Huber-White robust. The symbols *, **, and *** represent statistical significance at 10%, 5%, and 1%, respectively.

43. Third, Dr. Yermack incorrectly argues that the National Association of College and University Business Officers (NACUBO) endowment spending-rate data used in my analysis is unreliable because some institutions might not report their spending rates to NACUBO in all years. Yermack Rpt. at p. 23. Dr. Yermack provides no alternative spending-rate measure that should be preferred and no evidence that using alternative spending rates would have any effect on the estimates in the analysis. In reality, the effective spending rates reported by NACUBO for institutions with endowments of $1 billion or more are highly correlated with (a) those reported by the Defendant institutions to Congress; (b) those reported by Dr. Long in her expert report; and (c) those reported by Defendant institutions to NACUBO and in their own financial documents. A number of the Defendant institutions reported their endowment spending to Congress in 2008 and 2016, covering the period from 2003 to 2016 (not all institutions reported spending in all years). The average effective spending rates reported to Congress have a correlation of 0.92 with the averages reported by NACUBO. Dr. Long reports effective spending rates for 2009 to 2019, which have a correlation of 0.98 with the averages reported by NACUBO during the same years. And nearly all of the Defendant institutions reported institution-specific spending rates to NACUBO or in their own financial statements.[15] When these rates are compiled, I compute a correlation of 0.95 of these spending rates with the averages report by NACUBO between 1996 to 2022. In sum, the NACUBO averages that I used in my Initial Report are thus reliable measures of effective spending rates during between 1996 and 2022.

44. Fourth, Dr. Yermack argues that my use of a fixed spending rate to measure excess returns is incorrect, as spending rates vary over time and across institutions. Yermack Rpt. at p.

[15] Northwestern's spending rates were not reported consistently during this period and thus are excluded from the analysis.

4. Dr. Yermack does not present any evidence, however, that using a fixed spending rate produces biased estimates or that using a variable spending rate produces different results. I find that it does not. Dr. Yermack's proposal is flawed because it would introduce an endogenous explanatory variable. His proposal puts the same variable (spending) on both the left and right side of the regression equation, which is statistically invalid and can lead to biased estimates due to simultaneity (Wooldridge, 2002, p.51). In this case, and in my Initial Report, the variable of interest is spending on institutional aid. The independent or explanatory variable is excess returns above the typical spending rate. The purpose of the regression is to test whether excess returns have a statistically significant effect on institutional-aid spending. Using a variable spending rate to compute excess returns results in the inclusion of spending on both sides of the regression (as the outcome on the left, and as a determinant of excess returns on the right). One cannot put institutional aid spending on both sides of the regression without violating a fundamental principle of econometrics. (Wooldridge, 2009, p. 546; Stock and Watson, 2011, p. 326).

45. Dr. Yermack also proposes that the fixed spending rate of 4.7% might be too high and that 4.5% may be more appropriate. Yermack Rpt. at p. 23. Again, Dr. Yermack presents no empirical analysis to suggest that using a lower or higher fixed spending rate produces biased estimates or different results. If the correct spending rate were lower, then it would imply that institutions had even greater excess returns than those presented in my report. *Id*. at 29.

46. In addition, I have replicated my results using the alternatives that Dr. Yermack suggests: (a) lower and higher fixed effective spending rates of 4.5% and 4.9% (above the level used in my Initial Report) to test the sensitivity to such changes; (b) spending rates that vary across years; and (c) spending rates that vary across institutions (based on their average rates during the period of interest). Tables 7 and 8 present the estimates of regressions using excess returns based on

each of these alternative effective spending rates. Table 7 reveals that there is no evidence that higher endowment returns lead to greater aid spending or reduced net prices or effective institutional prices during the period of Challenged Conduct, which is consistent with the findings in my Initial Report. In contrast, Table 8 reveals large and statistically significant increases in aid and reductions in net price in response to returns prior to the period of the Challenged Conduct, which is also consistent with my results in my Initial Report. In short, there is no merit to Dr. Yermack's assertion that the results are sensitive to the effective spending rates against which excess returns are measured.

Table 7: Challenged Conduct Period: Effect of Lagged Excess Returns Using Alternative Spending Rates

	Institutional Grant Aid	Net Price	Effective Inst. Price
Eff. rate of 4.5%	-0.043	-0.008	-0.035
	(0.052)	(0.079)	(0.063)
Eff. rate of 4.9%	-0.046	-0.013	-0.041
	(0.058)	(0.087)	(0.070)
Eff. rate by year	-0.043	-0.011	-0.038
	(0.054)	(0.082)	(0.065)
Eff. rate by institution	-0.039	0.045	-0.001
	(0.044)	(0.074)	(0.058)

Notes: This table presents the effect of cumulative excess returns on the natural log of institutional grant aid, net price, and effective institutional price. The estimates are for 568 Group institutions during membership years. Estimates are based on a specification that includes institution fixed effects and year fixed effects. Standard errors are Huber-White robust. The symbols *, **, and *** represent statistical significance at 10%, 5%, and 1%, respectively.

Table 8: Prior to Challenged Conduct Period: Effect of Lagged Excess Returns Using Alternative Spending Rates

	Institutional Grant Aid	Net Price	Effective Inst. Price
Eff. rate of 4.5%	0.214***	-0.267***	-0.252***
	(0.064)	(0.094)	(0.063)
Eff. rate of 4.9%	0.217***	-0.271***	-0.256***
	(0.065)	(0.096)	(0.064)
Eff. rate by year	0.211***	-0.263***	-0.249***
	(0.063)	(0.093)	(0.062)
Eff. rate by institution	0.213***	-0.264***	-0.250***
	(0.063)	(0.090)	(0.061)

Notes: This table presents the effect of cumulative excess returns on the natural log of institutional grant aid, net price, and effective institutional price. The estimates are for 568 Group Defendants prior to the 2003-2004 or 2004-2005 academic years accounting for when they first engaged in the Challenged Conduct. Estimates are based on a specification that includes institution fixed effects and year fixed effects. Standard errors are Huber-White robust. The symbols *, **, and *** represent statistical significance at 10%, 5%, and 1%, respectively.

2. Dr. Yermack's Claims About Restricted Funding Do Not Undermine My Conclusion that Defendants Had Ample Endowment Resources to Increase Support for Institutional Aid

47. Fifth, in an apparent effort to contradict my conclusion about the financial ability of Defendants to devote greater support for institutional aid while maintaining real growth of their endowments, Dr. Yermack presents data for restricted and unrestricted assets in a year that is not representative of the period of the Challenged Conduct. Yermack Rpt. at p. B-1. In his Table 1, Dr. Yermack presents the share of assets that are restricted only in 2022. This is the last year of the period of Challenged Conduct and overstates the extent to which each institution faced restrictions on their endowment funds.

48. My Table 1, shown earlier, presents the share of assets that are unrestricted, temporarily restricted, and permanently restricted in each year from 2004 to 2022, which covers the period of the Challenged Conduct. My Table 9 below presents the average share of assets that were unrestricted, temporarily restricted, and permanently restricted for each institution between 2004 to 2022. From Table 1, I compute that, on average, more than 40% of assets are fully unrestricted during this period. Dr. Yermack's claim that 4 of 17 Defendant institutions had more than 80% of their endowments in restricted funds, Yermack Rpt. at p. 15, and thus less than 20% in unrestricted funds, based only on 2022 ignores the higher unrestricted fund share over the entire period of the Challenged Conduct. Indeed, over the entire period, only Georgetown lacked significant unrestricted funds.

Table 9: Restricted and Unrestricted Assets 2004-2022

	Percent of Assets: 2004-2022		
		Temporarily	Permanently
Institution	Unrestricted	Restricted	Restricted
Brown University	29%	35%	36%
California Institute of Technology	31%	27%	42%
Columbia University	52%	26%	22%
Cornell University	44%	25%	31%
Dartmouth College	27%	51%	22%
Duke University	61%	18%	22%
Emory University	51%	28%	22%
Georgetown University	-2%	33%	69%
Johns Hopkins University	42%	21%	37%
Massachusetts Institute of Technology	40%	43%	16%
Northwestern University	66%	20%	14%
Notre Dame	46%	36%	19%
Rice University	54%	29%	17%
University of Chicago	33%	40%	27%
University of Pennsylvania	55%	21%	23%
Vanderbilt University	60%	20%	20%
Yale University	25%	62%	13%

Notes: This table presents the share of assets that were unrestricted or restricted averaged across the years 2004 to 2022.

49. In addition, Dr. Yermack's invocation of restricted funding to discredit my analysis suffers from the same flaw I identified with Dr. Long's report in this respect. That is, just because institutions have restricted funds does not mean that those restrictions impair their ability to free up their operating funds for the very same purpose or other purpose, in this case institutional aid. Dr. Yermack argues that restricted endowment funds are dedicated to purposes that universities otherwise "would not" fund. Yermack Rpt. at p. 37. Some share of restricted funds, to be sure, are for purposes that institutions would not pursue on their own. Dr. Yermack's argument, however, is wrong in light of three important ways in which endowments can be used to increase aid. One, a significant fraction of restricted endowment funds is dedicated to financial aid. Two, institutions have significant unrestricted endowment components. And three, the largest categories of endowment spending are for categories that significantly align with institutional spending priorities, so there is likely to be substantial fungibility of assets. As I explained earlier in my response to Dr. Long on this subject, Paragraph No. 29, fungibility is consistent with the economics and law literature. Each of these types of endowment funds provides institutions with flexibility to leverage endowment revenue for institutional aid. Further, high investment returns, which increase the size of unrestricted endowment funds and funds dedicated to aid and other core priorities, can and should lead to greater spending on aid in the absence of offsetting reductions from other revenue sources.

50. Dr. Yermack asserts that my Initial Report "fails to properly account for the fact that many endowment funds are restricted and could not be devoted to undergraduate financial aid." Yermack Rpt. at p. 3. This is wrong. My report included the points detailed above concerning unrestricted funds, restricted funds dedicated to aid, and the fungibility of restricted funds with other revenue sources. Initial Report at pp. 24-26. In addition, my Initial Report examines the

capacity of institutions to increase institutional financial aid using only unrestricted funds. Initial Report Table 10 and p. 32.

51. Dr. Yermack references the implications of endowments being "spent down" had institutional aid increased, asserting that "[h]ad universities spent down their endowments more aggressively, for instance, it is possible that donors may have been reluctant to contribute new funds." Yermack Rpt. at p. 9. This is only a conjecture on his part, and in any event, it is implausible. Increasing aid generosity by 10% for all aid recipients during the period of Challenged Conduct from 2003 to 2022 would have reduced end-of-period endowment levels by just 3.9%. Initial Report at p. 31. Endowments would have grown by 134% even with the 10% increase in aid generosity. As noted in my response to Dr. Long, the 10% increase in aid could have been covered with less than 0.4% of unrestricted assets in any year. No spending of restricted funds would have been needed, even though a significant fraction of restricted funds is designated for aid. It is implausible that 134% endowment growth would trigger a change in new gifts due to possible concerns about imprudent spending out of endowments. Indeed, the slight difference in endowment levels over the nearly twenty-year period of the Challenged Conduct would have been hard for donors to perceive relative to overall endowment returns. Thus, the argument that increasing aid would deplete or "spend down" endowments, and restricted endowments in particular, leading to reduced future gifts, is implausible, and is not an implication of any of my tables. Similarly, Dr. Yermack's assertion that institutions could not increase financial aid without sacrificing other priorities, Yermack Rpt. at p. 36, is implausible in light of the magnitude of endowment growth relative to the cost of increasing aid detail above.

52. Dr. Yermack's assertion that endowment spending rates are inconsistent with the Defendants colluding on financial aid are wrong. Dr. Yermack argues that if the Defendants had colluded on financial aid, then spending rates from their endowments would need to move in lockstep and that they would be substantially lower than other universities. Yermack Rpt. at p.33. These arguments are irrelevant to my analysis and the implications of the Challenged Conduct, and Yermack presents no credible empirical analysis to support his claims. First, it is not clear why colluding on financial aid would lead to endowment spending rates moving in lockstep. The argument made in my report is not that institutions did not spend their endowments according to spending rules percentages, but rather that this spending did not lead to net increases in aid and reductions in prices for students. Endowment returns led to higher endowment levels and greater spending, but not to greater aid or reductions in net prices when Defendants were participating in the period of the Challenged Conduct. This implies that any additional revenue from the endowment is not being allocated to financial aid or is being offset by reductions to aid from other revenue sources. My findings do not preclude the possibility that these institutions are spending their endowments to increase net spending on other priorities.

53. Second, Dr. Yermack's empirical evidence is inadequate to draw conclusions about whether the Challenged Conduct changed spending from endowments. His empirical analysis is fundamentally flawed because he presents endowment spending only during the period from 2009 to 2022. Yermack Rpt. p. B-10 and B-11. Moreover, Dr. Yermack's Figure 1 presents endowment spending rates for just five of the Defendants from 2009 to 2022. Thus, no conclusions can be drawn about whether the Defendants changed their spending patterns before and during the Challenged Conduct. The data presented in Figure 1 do not cover the relevant period, nor do they include all of the Defendants: thus Figure 1 does not discredit my regression

analysis showing that Defendants reduced institutional aid spending and increase effective net prices when they were participating in the Challenged Conduct. Likewise, Dr. Yermack's Figure 2 presenting average spending rates for other higher education institutions in the U.S. for 2009 to 2018 does nothing to disprove my conclusions. Dr. Yermack provides no formal comparison to the Defendants, nor does he provide evidence allowing conclusions to be drawn about whether the Defendants changed their spending before and during the Challenged Conduct. Dr. Yermack's assertion that spending patterns are inconsistent with collusion have no merit. His evidence is inadequate to determine whether institutions changed their spending rates during the Challenged Conduct, while colluding on aid need not lead to a reduction in the overall spending from the endowment, since endowment funds can be used for other things.

C. Flaws in Dr. Hill's Critiques

54. Dr. Hill critiques my report in three ways. None are valid.

55. First, Dr. Hill critiques the use of standard errors in my statistical analysis, in an effort to discredit the statistical significance of my findings. Hill Rpt. at p. 132. I find that accounting for Dr. Hill's concern results in no change in the statistical significance of my findings. My statistical analysis documents that a 100% increase in excess endowment returns leads to 21.5% more generous aid and a 25.4% reduction in effective institutional price prior to the conduct, but a 4.4% reduction in aid and only a 3.8% reduction in effective institutional price during the period of the Challenged Conduct. Dr. Hill's assertion regarding standard errors has no bearing on the magnitudes of these estimates.[16] That is, the finding of no increase in aid or reduction in prices in response to endowment returns during the Challenged Conduct is unaffected by this

[16] Ordinary least squares regression estimates are unbiased in the presence of heteroskedastic or autocorrelated residuals. Dr. Hill's critique relates only to estimating the standard errors and has no effect on the sign or magnitude of the regression estimates themselves (Wooldridge 2002, p.p. 56-57).

critique. Dr. Hill's assertions pertain to whether or not the 25.9% and 21.6% gaps between the period before and during the Challenged Conduct are statistically significant given different assumptions about the structure of the residuals of the regression and the resulting standard errors. My Initial Report presents Heteroskedasticity Consistent standard errors. Heteroskedasticity occurs when the variance of the residuals is not constant across all levels of the independent variables (White, 1980).

56. Dr. Hill asserts that there is autocorrelation of residuals within institutions over time, which may result in the standard errors being bigger or smaller than they otherwise would be. Autocorrelated residuals refer to the fact that the deviations of the estimates of the dependent variable from its actual values ("residuals") are statistically correlated with each other over time, and not independent. Autocorrelation can be addressed, however, using Heteroskedasticity and Autocorrelation Consistent (HAC) standard errors (Newey and West, 1987). HAC standard errors are specifically designed to account for heteroskedasticity as well as autocorrelation of residuals over time.

57. Instead, Dr. Hill claims that only the use of "clustered" standard errors is justified, providing estimates of standard errors using this approach. Hill Rpt. at p. 136. Dr. Hill's use of clustered standard errors, however, is not supported by the autocorrelation test he conducts. Dr. Hill conducts a test for first-order autocorrelation, namely whether there is correlation in adjacent years (e.g., 2004 and 2005). He concludes from this that there is evidence of first-order autocorrelation. However, instead of estimating standard errors to address first-order autocorrelation, Dr. Hill uses clustered standard errors that are premised on the assumption that all residuals in the cluster (an institution in this case), for any year, are correlated, regardless of how far apart those years are. This is an extreme and baseless assumption, and amounts to

statistical overkill because it assumes that only residuals across universities, and not over time, are independent. Dr. Hill's use of clustered standard errors has the effect of treating the analysis as though it is based on only 17 independent observations, corresponding to the 17 Defendants, by assuming that residuals for all years of each Defendant are correlated. Using this technique can erroneously increase the estimated errors in the regression. The modern econometrics literature has highlighted that using clustered standard errors can produce estimates of the standard errors that are too large when the clusters represent a significant fraction of the population of interest, as is the case with Dr. Hill's method here, and when the number of observations per cluster is large (Abadie, Athey, Imbens, and Wooldridge 2023, Page 1).[17] Indeed, in this case, where all of the Defendants are alleged to have engaged in the Challenged Conduct, there is a single cluster of interest.[18]

58. In contrast, the approach that I present below, which accounts for heteroskedasticity and autocorrelation, directly addresses Dr. Hill's concern. My analysis of the autocorrelation of the residuals, using an Arellano-Bond test, only reveals evidence of autocorrelation for two or fewer

[17] Abadie et al. (2023) note that a common "misconception is that there is no harm in using clustering adjustments when they are not required, with the implication that if clustering the standard errors makes a difference, one should cluster." Page 3. They note that when used at inappropriate times, clustering can cause standard errors to be "severely inflated," Page 1. And that in certain cases "cluster standard errors may be unnecessarily large." Page 23. That is, implementing clustered standard errors, especially when they are not justified by empirical tests, can introduce bias in the size of the standard errors.

[18] Dr. Hill advanced this same clustered error critique to Dr. Singer's report, but I am confining my response to his critique of my analysis. My analysis is based on institution-year data, while Dr. Singer's report is based on structured data and a regression model designed to answer a different question. Thus, the appropriate options for estimating standard errors need not be the same across these analyses.

years.[19] My analysis disproves Dr. Hill's assertion that all years should be treated as correlated regardless of how far apart they are.

59. Dr. Hill's overboard use of clustered errors is apparent given the availability of, for example, Newey-West ("NW") standard errors that are heteroskedasticity and autocorrelation consistent and constitute a more targeted approach for addressing autocorrelation that does not introduce new sources of bias in the estimated standard errors.[20] The NW standard error approach allows the researcher to specify the nature of the autocorrelation in terms of the distance between years that are correlated. In this spirit, I calculate the standard errors of my regression under the assumption that there is autocorrelation across one, two, or three years. My Tables 10 and 11 below present the results. The estimates reveal that Heteroskedasticity Consistent standard errors confirm the 25.9% reduction in aid generosity after the onset of conduct is significant at the 99% level (the p-value is less than 0.01) and the increase in effective institutional prices is significant at the 98% level (the p-value is less than 0.02). The statistical significance of the change in aid generosity remains when using Heteroskedasticity and Autocorrelation Consistent standard errors that allow for autocorrelation with lags of one, two, or three years. Similarly, the 21.6% gap in effective institutional price remains significant at the 95% level when accounting for autocorrelation with lags of one, two, or three years. In sum,

[19] An Arellano-Bond tests for the presence of autocorrelation in the residuals across time periods (Arellano and Bond, 1991). The test reveals whether or not, for example, there is autocorrelation in the current year relative to 1 year prior, 2 years prior, etc. The results of the test reveal the nature of the autocorrelation that should be accounted for when estimating standard errors. The test for the regressions in my Initial Report does not reveal significant autocorrelation beyond two years.

[20] Newey-West standard errors account for heteroskedasticity as well as autocorrelation of residuals in adjacent years, without assuming that all residuals within an institution are correlated regardless of how far apart they are (Newey and West, 1987).

there is no evidence that adjusting standard errors to account for autocorrelation across years changes the statistical significance of my results in any material way.

Table 10: Heteroskedasticity and Autocorrelation Consistent Standard Errors: Institutional Aid

	Lag (0)	Lag (1)	Lag (2)	Lag (3)
Lagged Returns	0.215***	0.215***	0.215***	0.215***
p-value	(0.000)	(0.000)	(0.001)	(0.001)
Lagged Returns X Conduct	-0.259***	-0.259***	-0.259***	-0.259***
p-value	(0.002)	(0.003)	(0.005)	(0.007)

Table 11: Heteroskedasticity and Autocorrelation Consistent Standard Errors: Effective Institutional Price

	Lag (0)	Lag (1)	Lag (2)	Lag (3)
Lagged Returns	-0.254***	-0.254***	-0.254***	-0.254***
p-value	(0.000)	(0.000)	(0.000)	(0.000)
Lagged Returns X Conduct	0.216**	0.216**	0.216**	0.216**
p-value	(0.016)	(0.022)	(0.028)	(0.032)

60. In addition, my empirical design includes institution fixed effects and year fixed effects. Institution fixed effects ensure that the relationship between endowment returns and aid outcomes are based on changes over time within an institution (i.e., how aid at a university changes from year to year as the lagged excess returns on the endowment changes) (Stock and Watson, 2011, p.361).Year fixed effects ensure account for changes in the outcome that are common to all institutions in the same year (i.e., year fixed effects would account for an increase or decrease in institutional aid caused by a change in federal aid generosity that affects all universities). When implementing clustered standard errors, Dr. Hill only considers the

possibility that there might be correlation of residuals across years *within* institutions, and not the possibility that there might be correlation across institutions *within* years (Cameron, Gelbach, and Miller, 2011). Tables 12 and 13 below reveal that implementing clustered standard errors for institutions and years (two-way clustering) results in smaller standard errors and greater statistical significance. Most notably, fully clustering, rather than partially clustering, unravels Dr. Hill's conclusion that the change in price responsiveness is not statistically significant at the 95% level. Nonetheless, as noted above, attempting to use clustered standard errors in this context, with 17 institutions and 23 years, can produce unreliable standard errors. It is appropriate instead to address the specific nature of the suspected correlation, such as through heteroskedasticity and autocorrelation adjusted standard errors as presented above.

61. Overall, implementing standard errors that account for autocorrelation, and even those clustering across institutions and years, reveal that there was a statistically significant decrease in aid generosity and decrease in effective institutional prices in response to endowment returns after the onset of the Challenged Conduct.

Table 12: Clustering: Institutional Aid

		Cluster	Cluster
	Robust	(Inst.)	(Inst. and Year)
Lagged Returns	0.215***	0.215***	0.215***
p-value	(0.000)	(0.003)	(0.000)
Lagged Returns X Conduct	-0.259***	-0.259**	-0.259**
p-value	(0.002)	(0.037)	(0.021)

Table 13: Clustering: Effective Institutional Price

		Cluster	Cluster
	Robust	(Inst.)	(Inst. and Year)
Lagged Returns	-0.254***	-0.254***	-0.254***
p-value	(0.000)	(0.000)	(0.000)
Lagged Returns X Conduct	0.216**	0.216*	0.216**
p-value	(0.016)	(0.072)	(0.037)

62. Second, Dr. Hill incorrectly concludes that there is no relationship between endowment returns and endowment levels. Hill Rpt. at p. 140. My Initial Report examined the impact of excess investment returns on per-student aid and per-student prices. One mechanism through which excess returns might increase student aid is through greater endowment levels and greater spending per student, some of which is allocated to student aid, which would reduce student net prices. To highlight this mechanism, Tables 14 and 15 below present the effect of lagged excess returns on the natural log of lagged endowments, the natural log of lagged endowments per student, and the level of endowment per student (in dollars). This exercise reveals whether excess returns over time impact endowment levels (total endowment) and endowment per student (which captures an institution's per-student wealth and endowment spending power). I also present the effect of excess returns endowment per student in terms of natural logs, which reveals the change in endowment in percent terms. Each of these regressions reveals a large and statistically significant relationship between excess returns—the variable of interest—and endowment wealth. The results are statistically significant at the 99% level with and without accounting for autocorrelation of errors across years.

Table 14: Heteroskedasticity Consistent Standard Errors

		Log Lagged Endow	Lagged Endow
	Log Lagged Endow	Per FTE	Per FTE
Lagged Excess Returns	0.177***	0.221***	733,690***
	(0.044)	(0.050)	(59,390)

Table 15: Heteroskedasticity and Autocorrelation Consistent Standard Errors

		Log Lagged Endow	Lagged Endow
	Log Lagged Endow	Per FTE	Per FTE
Lagged Excess Returns	0.177***	0.221***	733,690***
	(0.066)	(0.073)	(89,566)

63. It is straightforward to further explore the mechanism through which excess returns should lead to more generous aid by considering the responsiveness of spending per student to excess returns.[21] If higher returns lead to greater spending per student, then an institution should also have the capacity to increase aid and reduce the net price per student. Tables 16, 17, and 18 below enable this exercise by presenting the effect of excess returns on per-student endowments, spending, aid, and net prices for the period of the Challenged Conduct. The estimates reveal clear evidence that, during the Challenged Conduct period, excess returns lead to larger endowments and greater spending, but not to greater aid or reduced net prices or effective institutional prices. That is, while participating in the Challenged Conduct, Defendant institutions spent more in response to excess returns (consistent with their endowment spending rules) but did not spend in such a way that increased institutional aid or reduced effective institutional prices. My estimates in this regard are presented from the onset of the Challenged Conduct through 2022. The

[21] Spending per student excludes hospitals and independent operations that are not core to the undergraduate mission. Spending includes, for example, instruction, academic support, student support, institutional support, and research.

relationship between excess returns and endowment levels and spending is even stronger when considering shorter periods during the Challenged Conduct, but continue to reveal no positive effects on aid or reductions in net prices.

Table 16: Heteroskedasticity Consistent Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.221***	0.174***	-0.044	-0.010	-0.038
	(0.050)	(0.030)	(0.055)	(0.083)	(0.066)

Table 17: Heteroskedasticity and Autocorrelation Consistent Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.221***	0.174***	-0.044	-0.010	-0.038
	(0.073)	(0.038)	(0.069)	(0.093)	(0.077)

Table 18: Clustered Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.221*	0.174***	-0.044	-0.010	-0.038
	(0.113)	(0.053)	(0.101)	(0.123)	(0.097)

64. Dr. Hill also asserts that institution fixed effects capture only time-invariant factors and cannot change when estimated for different time periods. Hill Rpt. at p. 242. He then uses these fixed effects to compare changes in aid generosity between the before and during Challenged Conduct periods. And finally, he asserts that an alternative model with a single institution effect produces a better estimate of the relationship between endowment returns and aid generosity. Hill Rpt. at p. 145. Each of these conclusions is based on a misunderstanding of what a fixed

effect is and how it can be interpreted, and misguided attempt to convert my regression equation into one designed for a different purpose (testing for changes in aid and price levels).

65. Dr. Hill incorrectly assumes that institution fixed effects capture only fixed institutional characteristics and cannot vary across time periods, and applies that assumption in his analysis. Hill Rpt. at p. 142. Specifically, Dr. Hill mistakes the interpretation of a fixed effect in a specific regression (a fixed value in that regression) as an unchanging value in the real world that cannot change over time. Fixed effects do not solely capture differences in the key variable of interest (here, effective price) due to fixed, time-invariant characteristics of institutions (such as urban or rural). They also capture all differences in the variable of interest across institutions that stem from average differences in time-varying characteristics that are otherwise not included in the regression equation.[22] For example, the effective price might be higher at an institution because it is in an urban area, but also because it serves, on average, higher-income students. This possibility reflects the fact that factors that vary over time, such as student composition, are also captured by institution fixed effects. Institution fixed effects also can vary across time periods due to multiple factors: (a) the impact of time-invariant fixed characteristics on the outcomes can change (e.g., the effect of being an urban campus on effective price is not likely to be the same in 2022 as it was in 2000); and (b) the time-varying factors that are captured by fixed effects also change over time (e.g., student composition can evolve and is unlikely to be the same across decades). Accordingly, institution fixed effects estimated for 2000-2003 will not be the same as

[22] This is clear if one considers a very simple regression model where an outcome of interest (effective price) is regressed on a fixed effect for each Defendant. The resulting fixed effect values will be the average effective price for each Defendant during the period covered by the data. Clearly average differences in price across institutions are not solely a function of fixed institutional characteristics, but also time-varying characteristics (such as the composition of the student population) that differ on average across these institutions.

those estimated for 2004-2022. Dr. Hill's misunderstanding of fixed effects leads to his incorrect assumption that the effects must be identical during before and during the Challenged Conduct.

66. Dr. Hill nonetheless applies his fixed effects analysis to conclude that prices were lower during the Challenged Conduct period than before, using 2004 as the baseline year for comparison (his Figures 47 and 48). Fixed effects across different regression equations, however, cannot be interpreted without accounting for the other variables included in the regressions. By definition, fixed effects can only be interpreted as the intercept of the regression (i.e., here, the value of the effective institutional price when the other variable in the regression is equal to 0). Dr. Hill's exercise ignores the levels of the other covariate, namely lagged excess returns, when attempting to identify a change in before and during Challenged Conduct generosity. If one takes excess endowment returns into consideration, then the average predicted effective institutional price in 2004 using the before and during models is identical: 10.32 in both models in 2004. Dr. Hill's exercise is even further flawed because it attempts to examine a change in effective price levels during the Challenged Conduct relative to before using a regression model that includes year fixed effects. Including year fixed effects in the model makes such a comparison impossible.[23]

67. A simple exercise highlights the flaw in Dr. Hill's approach. For this illustrative purpose, I now add another covariate to the regression, the number of undergraduate students attending each university. The inclusion of this covariate has no impact on the estimates of the baseline model. During the pre-conduct period, effective institutional prices decreases by 0.25% for each

[23] A model that includes a fixed effect for each year accounts for all shifts in effective price that occur across years. Thus, such a model already accounts for changes in effective price levels and makes it impossible to measure a shift in the level during the Challenged Conduct relative to before. Indeed, the fact that aid generosity and price will be identical in 2004 using the before and during regression models is ensured by the inclusion of a year fixed effect.

1% increase in excess endowment returns, and during the Challenged Conduct period the price has essentially no change in response to returns. However, replicating Dr. Hill's comparison of before- and during-conduct fixed effects now reveals an average pre-conduct fixed effect of 4.30, and a post-conduct fixed effect of 10.32. Interpreting this as evidence of a change in effective institutional prices would falsely imply a seismic increase in prices during the Challenged Conduct period. However, what this exercise really reveals is that fixed effects are not fixed in the sense that Dr. Hill assumes and they cannot be interpreted across regressions without accounting for the other covariates. Dr. Hill's attempt to conclude that my regression model shows that institutions reduced prices is based on a flawed interpretation of fixed effects.

68. Dr. Hill also proposes an inferior and inaccurate model to estimate the relationship between endowment returns and aid and effective institutional prices. Dr. Hill's proposal stems from his flawed assumptions that institution fixed effects cannot change between time periods (documented above) and his inappropriate attempt to convert my regression equation into one designed to test for level changes in aid generosity and prices. My Initial Report did not estimate whether or not there is a shift in aid and prices at the onset of Challenged Conduct. I understand that this is the purpose of Dr. Singer's analysis. However, Dr. Hill attempts to implement a design suited for a different question—whether there is a level shift in generosity—by forcing the institution fixed effect to be identical before and during the Challenged Conduct, based on the flawed understanding that fixed effects cannot change over time. Forcing the fixed effect to be unchanged before and during the Challenged Conduct period can introduce bias in the estimated relationship between investment returns and the outcomes of interest (institutional aid and effective institutional prices). Including separate fixed effects between periods allows my model to account for changes in time-varying characteristics across the Defendants and to more

credibly estimate the relationship between excess endowment returns and aid outcomes during the before and during conduct periods. The model in my Initial Report concludes that the effect of a 1% increase in excess endowment returns reduces effective institutional prices by a statistically insignificant 0.04% during the Challenged Conduct. This estimate is based on a model that focuses specifically on the Challenged Conduct period. Dr. Hill's model forces institution fixed effects during the Challenged Conduct to be the same as prior to the conduct and results in an estimated effect of endowment returns on effective institutional prices of 0.17%**.** His model does not account for changes in time-varying characteristics across institutions over time.

69. Beyond the issue of bias in Dr. Hill's proposed design, a comparison of the fit of the models reveals that mine fits the data substantially better than the one proposed by Dr. Hill. The adjusted R-squared of my model, a measure of how well the model fits the data that accounts for the number of variables it includes, is 0.765, relative to Hill's 0.7091. Another common, formal method of comparing models is the Akaike information criterion (AIC), which rewards models for fitting the data well and penalizes them for using more variables to do so (i.e., it examines how well the model fits the data while preferring simplicity). This criterion is used to test whether the inclusion of additional variables is justified on the grounds of a better fit of the data. This test reveals a score of -678.10 for my model relative to -591.17 for Dr. Hill's model, where a lower score is better. The evidence, in terms of both potential bias and overall fit, indicates that my model produces a better estimate of the relationship between investment returns and financial aid and prices.

D. Dr. Ammon's Critiques Are Without Merit

70. Dr. Ammon advances several arguments or critiques specific to Penn that are aimed at discrediting parts of my analysis. These arguments and critiques, too, are without merit.

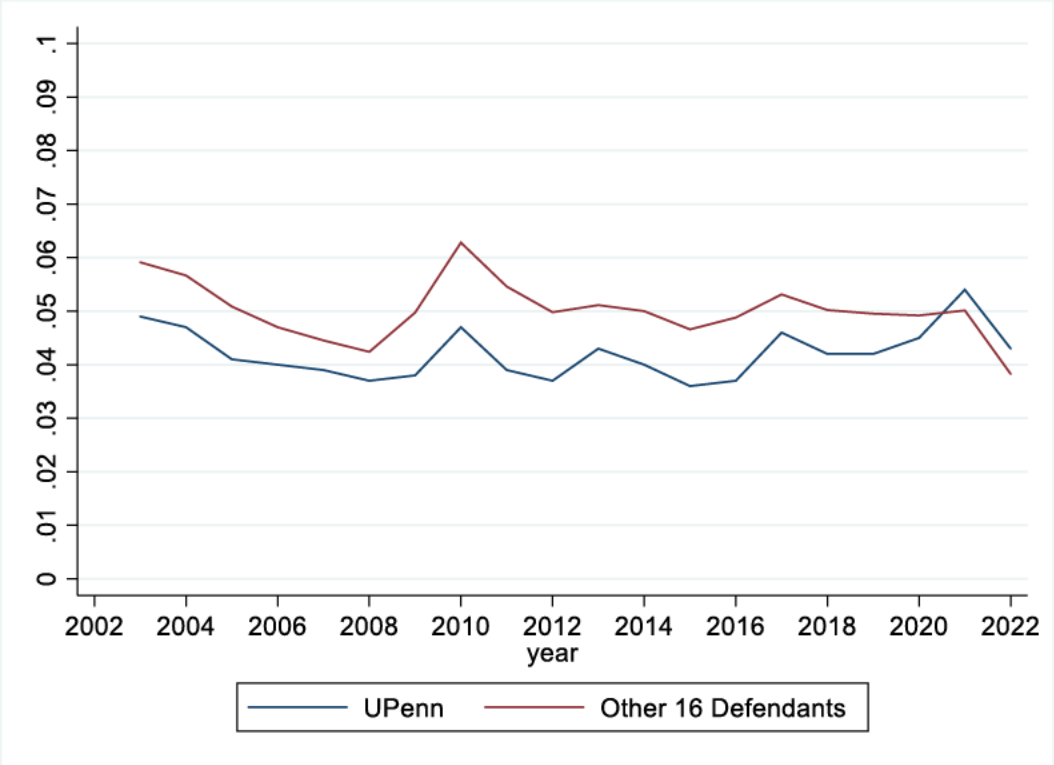
1. Dr. Ammon's Critiques Related to My Regression Analysis Are Without Merit

71. Dr. Ammon advances several Penn-specific critiques that appear to discredit my regression analysis. None does.

72. First, Dr. Ammon argues that Penn was especially generous with spending from the endowment from 1999 to 2019. Ammon Rpt. at p. 5. He apparently does so in an effort to discredit my statistical analysis. But Dr. Ammon is wrong. Figure 4 below presents the 17 Defendants' average effective spending rate from the endowment during the entire period of Challenged Conduct.[24] This figure reveals that the endowment spending rate at Penn was substantially below the average for the 17 Defendants and did not systematically increase during the period of the Challenged Conduct.

[24] The effective spending rate is the current year spending from the endowment divided by the endowment level at the start of the fiscal year.

Figure 4: Effective Spending Rate from the Endowment



Notes: The effective spending rate is the current endowment spending divided by the endowment value at the beginning of the fiscal year.

73. Second, Dr. Ammon argues that Penn has used its endowment to become exceedingly generous specifically for undergraduate students. Ammon Rpt. at p. 5. ("In aggregate, annual spending from the endowment in support of undergraduate financial aid more than quintupled over the period in question."). This claim is misleading because it fails to account for the historical pattern of list tuition prices at Penn. Spending from the endowment on aid can be offset by reductions in spending from other sources and by increases in the list prices charged to students. Accordingly, the right way to examine generosity is take account of how list prices and discounts interact to shape net revenue collected per student at particular points in time, as well as how endowment size and net revenue have evolved over the period of the Challenged Conduct. Table 19 reports the results of such an analysis (as well as replicating the analysis in my Initial Report

relating to this exercise) for Penn. Table 19 shows that Penn increased its real revenue per undergraduate student in a fashion very similar to that for all Defendants. From 2003 to 2022, Penn's revenue per student, adjusted for higher education inflation using the Higher Education Price Index ("HEPI"), increased from $49,607 to $54,029. This increase compares to the increase of $45,312 to $49,488 averaged across all Defendants. Indeed, Penn collected more revenue per student than the average Defendant institution throughout the Challenged Conduct period, as well as increased its average revenue collected by amounts nearly identical in magnitude to the average of the other Defendants during the period of Challenged Conduct. The increase in Penn's real revenue per student stems from increasing list prices. As shown in Table 20 below, for first-time, full-time students, the increase in inflation-adjusted total charges exceeded the increase in discounts, leading to increased real revenue. The size of the real, inflation-adjusted, endowment is included to provide perspective about the magnitude of institutional aid for freshman students. Dr. Ammon's claims of Penn's supposed generosity thus must be discounted by the fact that Penn's increased aid was offset by its price increases. As a result, there is no evidence that, when prices and aid are considered together, Penn was more generous with its undergraduate students than other Defendants, or more generous over time.

Table 19: Per-Student Revenue

	Tuition and Room and Board		
Academic Year		CPI	HEPI
2003	29,092	46,272	49,607
2004	30,080	46,602	49,476
2005	31,660	47,442	50,107
2006	33,758	49,005	50,831
2007	35,560	50,202	52,063
2008	36,438	49,529	50,829
2009	35,917	48,995	49,008
2010	36,177	48,553	48,925
2011	36,445	47,416	48,159
2012	38,138	48,613	49,571
2013	38,118	47,886	48,780
2014	40,433	49,984	50,242
2015	42,986	53,076	52,355
2016	43,172	52,642	51,787
2017	45,817	54,702	53,332
2018	47,976	55,914	54,399
2019	47,639	54,533	52,472
2020	49,829	56,345	53,841
2021	52,439	56,636	55,161
2022	54,029	54,029	54,029

Notes: Tuition, room, and board revenue per undergraduate student is list price tuition, room, and board less average institutional aid per undergraduate. The values are adjusted by fiscal year CPI and the Commonfund Higher Education Price Index.

Table 20: Total Revenue from Full-Time, First-Time Students (in millions of dollars)

	First-Time, Full-Time Students			
Year	List Cost	Inst. Aid	Revenue	Endowment
2004	140	29	111	6,226
2005	141	28	112	6,548
2006	151	29	122	7,713
2007	145	28	117	9,367
2008	147	31	116	8,473
2009	158	40	118	7,053
2010	161	45	117	7,608
2011	160	48	112	8,563
2012	166	49	117	8,610
2013	169	54	115	9,725
2014	171	53	118	11,846
2015	177	53	125	12,512
2016	184	59	125	13,066
2017	191	59	133	14,582
2018	193	58	135	16,057
2019	200	66	134	16,770
2020	196	64	132	16,823
2021	190	60	129	22,166
2022	185	58	126	20,724

Notes: List Cost is the sum of tuition and room and board charged to each first-time, full-time student. Institutional aid is the sum of institutional aid for each first-time, full-time student. Revenue is the difference between the List Cost and Institutional Aid. Each variable is inflation adjusted to 2022 dollars using the CPI.

74. Third, like Dr. Yermack, Dr. Ammon incorrectly claims that the lack of a relationship between aid and endowment returns stems from the Financial Crisis. Ammon Rpt. at p. 6. Dr. Ammon's claim stems from a lack of understanding of the role of year fixed effects in a regression specification. Specifically, he asserts that my report "fails to consider that universities might have purposefully increased their financial aid spending in the wake of the global financial crisis, a behavior pattern that would show an inverse relationship between trailing returns and spending." Dr. Ammon's assertion is wrong. Specifically, each regression specification in my Initial Report includes year fixed effects that capture changes in aid and prices in each year that

are common across all Defendants. Each year fixed effect accounts for changes in aid and net prices and effective institutional prices that are common to each of the Defendants, such as those resulting from stock market losses and changes in students' financial situations during the Financial Crisis. That is, I included year fixed effects in the specification specifically to ensure that the results were not driven by the dynamics described by Dr. Ammon. The lack of a relationship between aid and prices and excess endowment returns in my initial report is not due to the Financial Crisis, but instead reflects the statistically valid result that institutions with higher returns were not allocating their excess returns to institutional aid during the years in which they participated in the Challenged Conduct**.**

75. Tables 5 and 6 above examine the impact of endowment returns on institutional aid and effective institutional price before and after the Financial Crisis, respectively. The estimates reveal that the lack of an effect of returns is evident when restricting attention to cohorts enrolled prior to and after the crisis, respectively. The results shown in both tables demonstrate that the estimates for the full period of Challenged Conduct are not driven by cohorts enrolled during the Financial Crisis.[25] Dr. Ammon's assertion that the regression results are driven by the Financial Crisis is wrong.

76. Fourth, Dr. Ammon asserts that the excess returns for Penn reported in Table 9 of my report are entirely due to the high returns experienced in 2021. In fact, Penn had high excess

[25] Table 5 reveals, for example, that there is no significant impact of investment returns on student aid or net prices at Defendant institutions when restricting attention to cohorts entering college during the Challenged Conduct but prior to the Financial Crisis (e.g., for the 2003-2004 to 2007-2008 cohorts). Likewise, there is no significant impact of returns on student aid or net prices when examining post-Financial Crisis cohorts such as 2012-2013 through 2021-2022. These results reveal that the lack of a relationship between returns and the outcomes of interested during the Challenged Conduct period is not a result of including cohorts that were enrolled at the Defendants during the Financial Crisis.

returns prior to the large gains in that year. For example, the university's excess returns during the Challenged Conduct were $1.9 billion as of 2015, $2.5 billion as of 2018, and $4.0 billion as of 2022. While the returns of 2021 certainly increased the total, excess returns were very large prior to 2021.

2. Dr. Ammon's Critiques Aimed at My Conclusion that Penn Could Have Afforded to Provide More Institutional Aid Are Wrong

77. Dr. Ammon makes several arguments attempting to discredit the analysis in my Initial Report that Penn had sufficient endowment resources to have increased its institutional aid spending and still have generated real growth in its endowment. None has merit.

78. First, like other Defendants' experts who cited restricted funds in the endowment as a reason why Defendants could not have increased their aid funding, Dr. Ammon highlights the restricted endowments at Penn, asserting that that 7,695 of 8,700, or 88%, of endowment funds at Penn are restricted. Dr. Ammon overstates the extent to which Penn's endowment funds are restricted. Dr. Ammon's statistics do not capture the size of each of these funds and therefore are not informative about the share of the university's total endowment assets that are restricted and unrestricted. Indeed, the sheer number of funds suggests that a large number are small funds (e.g., a single chaired professorship). In contrast, the report of Dr. Yermack states that 62% of Penn's assets were unrestricted in 2022 (Table 1), the highest fraction for any Defendant institution in that year. Table 21 below presents IPEDS asset data between 2004 and 2022, confirming that, in 2022, 62% of assets at Penn were unrestricted. While Penn has assets beyond those in the endowment, financial reports for 2022 indicate that 51.5% of Penn's endowment assets were unrestricted. Penn reported unrestricted endowment assets of $5.9 billion in 2017, $7.3 billion in 2019, and $10.7 billion in 2022 (Penn Financial Reports, 2016-2017, 2018-2019,

2021-2022).[26] Accordingly, simply counting the number of funds that are restricted or not, as Dr. Ammon does, produces a highly biased measure of the extent to which the university has unrestricted assets in its endowment. Beyond this, as I have noted in my response to other Defendant experts' who have pointed to restricted funds, restricted funds are often dedicated to undergraduate aid, while others (such as chaired professorships) offset spending from general fund sources, freeing up those funds for other purposes, including institutional aid.

Table 21: Restricted and Unrestricted Assets for Penn

		Assets (in Billions of dollars)				Percent of Assets		
				Temporarily	Permanently		Temporarily	Permanently
Year	Endowment	Total	Unrestricted	Restricted	Restricted	Unrestricted	Restricted	Restricted
2004	4,019	5,888	2,872	1,422	1,594	49%	24%	27%
2005	4,370	6,388	3,087	1,553	1,748	48%	24%	27%
2006	5,313	7,323	3,612	1,757	1,954	49%	24%	27%
2007	6,635	8,713	4,274	2,314	2,125	49%	27%	24%
2008	6,233	8,846	4,466	2,101	2,279	50%	24%	26%
2009	5,171	7,561	3,851	1,375	2,336	51%	18%	31%
2010	5,669	8,229	4,258	1,516	2,455	52%	18%	30%
2011	6,582	9,830	5,103	2,107	2,620	52%	21%	27%
2012	6,755	9,694	4,914	2,018	2,762	51%	21%	28%
2013	7,741	11,294	5,933	2,434	2,927	53%	22%	26%
2014	9,582	12,972	6,869	2,960	3,142	53%	23%	24%
2015	10,134	13,491	7,153	3,027	3,311	53%	22%	25%
2016	10,715	14,513	8,447	2,630	3,435	58%	18%	24%
2017	12,213	16,247	9,467	3,108	3,673	58%	19%	23%
2018	13,777	18,574	11,153	3,554	3,867	60%	19%	21%
2019	14,650	19,425	11,392	3,823	4,210	59%	20%	22%
2020	14,877	19,566	11,366	3,779	4,421	58%	19%	23%
2021	20,524	26,949	15,904	6,364	4,681	59%	24%	17%
2022	20,724	28,875	17,808	6,091	4,976	62%	21%	17%

Notes: Endowments and assets for Penn only. The values are presented in nominal dollars. Endowment data are based on values reported by institutions to NACUBO. Restricted and unrestricted assets are based on data reported by institutions to the National Center for Education Statistics IPEDS.

[26] Moreover, as Penn has admitted in this case: "since 2003, Penn has understood itself to be free from any legal restriction on its ability to use Unrestricted Endowment Funds to fund "Need-Based Aid." Response to Request for Admission No. 20.

79. Second, Dr. Ammon makes a similarly uninformative claim about the distribution of endowment funds across the university. He notes: "At Penn, the vast majority of endowment units are owned by one of Penn's twelve schools (the majority of which do not have undergraduate students)." Ammon Rpt. at p. 4. But, as a matter of logic, Ammon's observation that the majority of Penn's twelve schools do not have undergraduate students does not imply or show that the majority of endowment funds or funds dedicated to aid are held at schools without undergraduate students. Moreover, his reference is to "endowment units," not to the dollars of the endowments owned by each of Penn's individual schools.

80. Third, Dr. Ammon claims further that Penn could not have increased spending "from endowment units dedicated to financial aid without eroding long-term purchasing power." Ammon Rpt. at p. 12. My Initial Report says nothing about Penn increasing spending on aid while only using those funds "dedicated to financial aid." Such an analysis would grossly understate Penn's capacity to increase aid funding by ignoring that more than half of their assets are unrestricted (see above). Dr. Ammon's critique is an attempt to reframe the question as a tautological one where the institution could not be substantially more generous with aid if it chooses to restrict revenue to the funds already dedicated to that purpose. This is clear in the figure on page 12 of Dr. Ammon's report, where it is shown that a fund that is dedicated to aid and not increasing in value could not sustain increased withdrawals. Dr. Ammon's critique has no bearing on the broader question of the impact of increasing aid generosity through the endowment as a whole. That analysis (Table 10 of my report) reveals that Penn could have increased aid by 10% for the entire period from 2003 to 2022, with no offsetting reductions in spending elsewhere, with little impact on the value of its endowment, and comfortably within the confines of unrestricted funds. Table 22 below further documents the amount that would be needed to increase aid generosity by

10% as a percentage of Penn's overall endowment and as percent of Penn's unrestricted assets. The table reveals that, for example, increasing aid for all students by 10% would represent less than a quarter of one percent of the value of their unrestricted assets in the typical year between 2017 and 2022. That is, the argument that Penn's "financial aid units would have ended the period at only 60% of their beginning values" and would have "eroded long-term purchasing power" by increasing aid generosity hinges on the misperception that endowment funds are entirely restricted and thus that increases would need to solely come from funds dedicated to aid. Ammon Rpt. at p. 13.

Table 22: Cost to Increase Aid Relative to Endowment and Unrestricted Asset Levels

		Unrestricted		10% Inst. Aid as Percent	
Year	Endowment	Assets	10% Inst. Aid	Endowment	Unr. Assets
2004	4,019	2,872	9	0.21%	0.30%
2005	4,370	3,087	9	0.20%	0.28%
2006	5,313	3,612	9	0.17%	0.25%
2007	6,635	4,274	9	0.14%	0.21%
2008	6,233	4,466	11	0.17%	0.24%
2009	5,171	3,851	13	0.26%	0.35%
2010	5,669	4,258	15	0.27%	0.36%
2011	6,582	5,103	17	0.26%	0.33%
2012	6,755	4,914	17	0.26%	0.35%
2013	7,741	5,933	20	0.25%	0.33%
2014	9,582	6,869	20	0.21%	0.29%
2015	10,134	7,153	20	0.19%	0.28%
2016	10,715	8,447	22	0.21%	0.26%
2017	12,213	9,467	22	0.18%	0.23%
2018	13,777	11,153	23	0.16%	0.20%
2019	14,650	11,392	26	0.18%	0.23%
2020	14,877	11,366	27	0.18%	0.23%
2021	20,524	15,904	25	0.12%	0.16%
2022	20,724	17,808	26	0.13%	0.15%

Notes: Endowments, assets, and aid for Penn only. The values are presented in nominal dollars.

E. My Empirical Analyses Are Robust to Alternative Years of 568 Membership

81. Counsel has requested that I replicate my analyses under the assumption that Rice was not engaging in the Challenged Conduct for the 2012-2013, 2013-2014, and 2014-2015 cohorts, and the Emory was engaging in the Challenged Conduct for the 2012-2013 cohort. Appendix Tables A1-A5 present estimates from my Initial Report under these new assumptions about the cohorts affected by the Challenged Conduct. Tables A1 and A2 show that the regression analyses are nearly unchanged, with no evidence that higher endowment returns lead to greater spending on institutional aid or reduced effective institutional prices. Consistent with my Initial Report, Table A3 shows that if Rice University's endowment would have been only 2.9% in 2022 had it increased aid generosity by 10% during every year of participation in the Challenged Conduct. Tables A6, A7, and A8 replicate the estimates from Tables 5, 6, and 7 above, and indicate no meaningful change. Likewise, Tables A9 through A17 examine the robustness of Tables 10 through 18 above to the alternative Challenged Conduct dates. The results are essentially unchanged, with the estimates revealing clear evidence of reduced aid and increased net prices and effective institutional prices during the Challenged Conduct relative to before. The results are robust to alternative methods of estimating the standard errors.

F. Conclusion

82. My Initial Report concluded that the Defendants had large endowment investment returns and growth during the Challenged Conduct period, that the Defendants did not use these returns to increase institutional aid or reduce effective institutional prices, and that the Defendants could have easily provided more generous aid while sustaining endowment growth and spending. None of the critiques posed by the Defendants' Experts change these conclusions. None of the experts disputes the findings that the Defendants had cumulative investment returns of 479% between 2003 and 2022 and real, higher-education inflation adjusted endowment growth of 144%.

83. Each of the Defendants' Experts' critiques of my analyses is wrong. My Initial Report shows that the Defendants did not increase aid and reduce prices in response to higher investment returns. Defendants' Experts assert that the Defendants could not have increased support for aid due to endowment restrictions. Endowments include substantial shares of unrestricted assets, restricted assets dedicated to aid, and restricted assets that are fungible with other revenue sources (e.g., for chaired professorships). Each of these factors allows the Defendants to increase aid in response to cumulative investment returns and endowment growth. They did not. This is true both for all Defendants and for Penn specifically.

84. I have shown that Defendants' Experts critiques of my regression analysis are wrong. Defendants' Experts incorrectly identify the cohorts affected by the Financial Crisis. Defendants' Experts present a number of empirical analyses focused on 2009 to 2022 that do not capture the period of the Challenged Conduct and cannot identify changes due to the Challenged Conduct. I show that the results in my Initial Report are not driven by the Financial Crisis: my findings hold both before and after the Financial Crisis. Further, the statistical significance of my estimates is robust to alternative methods of computing standard errors. My results show that when the Defendants had higher cumulative investment returns, they spent more from their endowments due to endowment spending rules, but that this spending did not result in increased financial aid or reduced prices for students. I have shown that these results are robust to alternative methods of computing institutions excess returns above spending and inflation. The Defendants' Experts' critiques are without merit and do not change the conclusions of my Initial Report.

George Bulman, Ph.D.:

Executed on October 7, 2024

APPENDIX A: MATERIALS RELIED UPON

Expert Reports

Dr. Bridget Terry Long, dated August 7, 2024
Dr. David L. Yermack, dated August 7, 2024
Dr. Nicholas Hill, dated August 7, 2024
Dr. Peter Ammon, dated August 7, 2024

Financial Reports

Duke University Financial Statements 2022/2023.

University of Notre Dame - Annual Financial Report, 2022.

University of Pennsylvania - Annual Financial Report, 2016-2017.

University of Pennsylvania - Annual Financial Report, 2018-2019.

University of Pennsylvania - Annual Financial Report, 2021-2022.

Articles and Literature

A. Colin Cameron, Jonah B. Gelbach, and Douglas L. Miller, *Robust inference with multiway clustering*, JOURNAL OF BUSINESS & ECONOMIC STATISTICS, 29.2: 238-249. (2011).

Alberto Abadie, Susan Athey, Guido W Imbens, Jeffrey M Wooldridge, *When Should You Adjust Standard Errors for Clustering?* THE QUARTERLY JOURNAL OF ECONOMICS, Volume 138, Issue 1: 1–35 (2023).

Andrew C. Eggers, Guadalupe Tuñón, and Allan Dafoe, *Placebo tests for causal inference*, AMERICAN JOURNAL OF POLITICAL SCIENCE 68.3: 1106-1121 (2024).

Burton A. Weisbrod, and Evelyn D. Asch, *Endowment for a Rainy Day*, STANFORD SOCIAL INNOVATION REVIEW (2010).

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Halbert White, *A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity*, ECONOMETRICA, vol. 48, no. 4: pp. 817–38 (1980).

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Nick Huntington-Klein, *The effect: An introduction to research design and causality*, CHAPMAN AND HALL/CRC (2021).

Peter Conti-Brown, *Scarcity Amidst Wealth: The Law, Finance, and Culture of Elite University Endowments in Financial Crisis*, STANFORD LAW REVIEW 63 (3): 699-749 (2011).

Ronald G. Ehrenberg, *Demystifying Endowments*, TIAA-CREF INSTITUTE ADVANCING HIGHER EDUCATION,Working Paper (2009).

Stock James, and Mark Watson, *Introduction to Econometrics*, 3rd Edition Addison Wesley Longman (2011).

Susan Athey, and Guido W. Imbens, *The State of Applied Econometrics: Causality and Policy Evaluation*, JOURNAL OF ECONOMIC PERSPECTIVES, 31 (2): 3–32 (2017).

Whitney K. Newey, and Kenneth D. West, *A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix*, ECONOMETRICA, vol. 55, no. 3: pp. 703–08 (1987).

APPENDIX B

Table A1: During Years in Which Defendants Engaged in Challenged Conduct

	Institutional Grant Aid	Net Price	Effective Inst. Price
Lagged Excess Returns	-0.044	-0.011	-0.039
	(0.055)	(0.083)	(0.066)
Mean Dep	10.61	10.13	10.22
Observations	243	243	243

Table A2: During the Challenged Conduct: Robustness

	Institutional Grant Aid	Net Price	Effective Inst. Price
Baseline			
Lagged Excess Returns	-0.044	-0.011	-0.039
	(0.055)	(0.083)	(0.066)
Controlling for Undergraduate Enrollment			
Lagged Excess Returns	-0.044	-0.011	-0.039
	(0.055)	(0.083)	(0.066)
Baseline Endowment Per Student X Year			
Lagged Excess Returns	-0.136*	0.141	0.065
	(0.078)	(0.096)	(0.074)
Need-Based Aid Scaling			
Lagged Excess Returns	-0.044	-0.011	-0.039
	(0.055)	(0.083)	(0.066)

Table A3: Impact of More Generous Aid on Endowment Levels (in millions)

	Endowments (2022 dollars)		Hypothetical 2022 Endowments With Greater Aid				Max With Unrestricted
Institution	2003	2022	Plus 10%	Plus 20%	Full Tuition	Full Cost	2022
Brown University	2,492	6,141	5,897	5,653	5,343	4,472	36%
Columbia University	7,417	13,280	12,690	12,100	12,022	10,173	73%
Cornell University	4,868	9,838	8,963	8,088	6,552	2,828	19%
Dartmouth College	3,617	8,066	7,714	7,362	7,500	6,287	52%
Duke University	5,145	12,116	11,545	10,974	11,028	9,079	62%
Emory University	6,854	9,998	9,787	9,577	8,898	7,922	105%
Georgetown University	1,008	3,210	2,859	2,508	1,863	286	38%
Massachusetts Institute of Tecl	8,754	24,740	24,202	23,665	23,641	21,643	133%
Northwestern University	5,203	14,121	13,496	12,870	11,552	8,791	84%
Rice University	5,009	7,814	7,585	7,355	7,163	6,138	145%
University of Chicago	5,494	9,915	9,674	9,433	8,571	7,420	98%
University of Notre Dame	4,388	16,729	15,971	15,213	13,657	10,684	90%
University of Pennsylvania	6,049	20,724	19,968	19,212	18,548	15,717	141%
Vanderbilt University	3,443	10,206	9,610	9,014	9,255	6,942	103%
Yale University	18,816	41,383	41,137	40,891	41,001	40,125	280%

Table A4: More Generous Aid and Endowment Levels: Reduced Risk

	Endowments (2022 dollars)		Hypothetical 2022 Endowments (assuming 10% lower returns)			
Institution	**2003**	**2022**	**Plus 10%**	**Plus 20%**	**Full Tuition**	**Full Cost**
Brown University	2,492	6,141	4,864	4,647	4,374	3,602
Columbia University	7,417	13,280	10,162	9,615	9,570	7,870
Cornell University	4,868	9,838	7,585	6,771	5,381	1,937
Dartmouth College	3,617	8,066	6,239	5,918	6,052	4,948
Duke University	5,145	12,116	9,228	8,708	8,776	7,007
Emory University	6,854	9,998	8,203	8,012	7,398	6,515
Georgetown University	1,008	3,210	2,620	2,290	1,693	216
Massachusetts Institute of Tecl	8,754	24,740	18,799	18,319	18,308	16,526
Northwestern University	5,203	14,121	11,205	10,628	9,453	6,921
Rice University	5,009	7,814	5,998	5,786	5,626	4,692
University of Chicago	5,494	9,915	8,013	7,793	7,008	5,958
University of Notre Dame	4,388	16,729	13,250	12,563	11,185	8,496
University of Pennsylvania	6,049	20,724	17,367	16,679	16,103	13,535
Vanderbilt University	3,443	10,206	8,486	7,935	8,168	6,033
Yale University	18,816	41,383	31,654	31,431	31,553	30,768

Table A5: Impact of More Generous Aid on Endowment Levels: 10-Year Projection

	Average Returns (2022 Dollars)			Average Returns -2% (2022 Dollars)			Fixed Returns -2% (2022 Dollars)		
Institution	2032	Plus 10%	Plus 20%	2032	Plus10	Plus20	2032	Plus10	Plus20
Brown University	9,823	9,312	8,801	8,147	7,720	7,294	7,807	7,412	7,016
California Inst of Tech	5,016	4,955	4,895	4,148	4,094	4,040	4,621	4,562	4,502
Columbia University	17,953	16,281	14,609	14,841	13,412	11,984	16,883	15,471	14,058
Cornell University	14,177	11,860	9,542	11,733	9,753	7,773	12,507	10,374	8,241
Dartmouth College	12,240	11,292	10,344	10,141	9,335	8,530	10,254	9,487	8,719
Duke University	18,923	17,317	15,711	15,685	14,324	12,963	15,404	14,213	13,022
Emory University	12,134	11,794	11,453	10,010	9,727	9,445	12,710	12,369	12,028
Georgetown University	5,877	5,161	4,444	4,886	4,281	3,676	4,081	3,348	2,615
Johns Hopkins University	14,157	14,157	14,157	11,756	11,756	11,756	10,481	10,481	10,481
Massachusetts Inst of Tech	42,531	40,978	39,425	35,318	34,003	32,688	31,452	30,357	29,262
Northwestern University	23,765	22,089	20,412	19,727	18,299	16,871	17,953	16,514	15,076
Rice University	9,826	9,138	8,450	8,111	7,521	6,931	9,934	9,339	8,743
University of Chicago	13,461	13,022	12,584	11,128	10,764	10,399	12,605	12,214	11,823
University of Notre Dame	33,667	31,470	29,272	28,040	26,174	24,308	21,268	19,640	18,012
University of Pennsylvania	39,426	38,005	36,585	32,802	31,620	30,438	26,347	25,121	23,894
Vanderbilt University	17,992	16,885	15,778	14,948	14,027	13,106	12,975	12,009	11,042
Yale University	62,347	61,305	60,263	51,645	50,743	49,842	52,611	51,835	51,059

Table A6: Regression Results Using Alternative Ending Cohorts

Sample (last cohort)	Inst. Grant Aid		Net Price		Effective Inst. Price	
2004-2005	-0.282	(0.555)	0.483	(0.724)	0.405	(0.576)
2005-2006	-0.022	(0.242)	-0.242	(0.288)	-0.214	(0.261)
2006-2007	0.014	(0.123)	-0.245*	(0.136)	-0.213**	(0.105)
2007-2008	-0.113	(0.102)	-0.009	(0.117)	-0.019	(0.090)
2008-2009	-0.120	(0.074)	-0.049	(0.088)	-0.002	(0.072)
2009-2010	-0.119*	(0.071)	-0.060	(0.084)	-0.006	(0.070)
2010-2011	-0.136*	(0.071)	-0.034	(0.082)	0.017	(0.070)
2011-2012	-0.147**	(0.071)	-0.001	(0.083)	0.041	(0.070)
2012-2013	-0.149**	(0.068)	0.035	(0.083)	0.067	(0.071)
2013-2014	-0.150**	(0.065)	0.071	(0.084)	0.092	(0.070)
2014-2015	-0.116*	(0.061)	0.048	(0.084)	0.061	(0.069)
2015-2016	-0.091	(0.057)	0.064	(0.074)	0.058	(0.060)
2016-2017	-0.064	(0.053)	0.073	(0.064)	0.049	(0.051)
2017-2018	-0.041	(0.049)	0.045	(0.065)	0.015	(0.053)
2018-2019	-0.025	(0.044)	-0.019	(0.070)	-0.039	(0.055)
2019-2020	-0.010	(0.040)	-0.061	(0.064)	-0.079	(0.051)
2020-2021	-0.045	(0.071)	0.007	(0.102)	-0.031	(0.083)
2021-2022	-0.044	(0.055)	-0.011	(0.083)	-0.039	(0.066)

Table A7: Regression Results Using Alternative Starting Cohorts

Sample (first cohort)	Inst. Grant Aid		Net Price		Effective Inst. Price	
2003-2004	-0.044	(0.055)	-0.011	(0.083)	-0.039	(0.066)
2004-2005	-0.026	(0.060)	-0.012	(0.090)	-0.044	(0.072)
2005-2006	-0.019	(0.067)	0.009	(0.100)	-0.027	(0.080)
2006-2007	-0.008	(0.076)	0.020	(0.118)	-0.025	(0.093)
2007-2008	0.017	(0.085)	0.010	(0.134)	-0.037	(0.106)
2008-2009	0.023	(0.090)	0.023	(0.144)	-0.031	(0.112)
2009-2010	0.031	(0.091)	-0.001	(0.148)	-0.053	(0.115)
2010-2011	0.040	(0.092)	-0.027	(0.152)	-0.075	(0.118)
2011-2012	0.029	(0.098)	-0.047	(0.163)	-0.075	(0.127)
2012-2013	0.008	(0.107)	-0.031	(0.184)	-0.059	(0.142)
2013-2014	-0.013	(0.120)	-0.040	(0.208)	-0.045	(0.161)
2014-2015	-0.048	(0.132)	-0.009	(0.237)	0.001	(0.179)
2015-2016	-0.049	(0.141)	-0.023	(0.261)	0.005	(0.196)

Table A8: Challenged Conduct Period: Effect of Lagged Excess Returns Using Alternative Spending Rates

	Institutional Grant Aid	Net Price	Effective Inst. Price
Eff. rate of 4.5%	-0.042	-0.009	-0.037
	(0.052)	(0.078)	(0.063)
Eff. rate of 4.9%	-0.045	-0.014	-0.042
	(0.058)	(0.087)	(0.070)
Eff. rate by year	-0.042	-0.012	-0.039
	(0.054)	(0.082)	(0.065)
Eff. rate by institution	-0.038	0.044	-0.002
	(0.044)	(0.074)	(0.058)

Table A9: Heteroskedasticity and Autocorrelation Consistent Standard Errors: Institutional Aid

	Lag (0)	Lag (1)	Lag (2)	Lag (3)
Lagged Returns	0.215***	0.215***	0.215***	0.215***
p-value	(0.000)	(0.000)	(0.001)	(0.001)
Lagged Returns X Conduct	-0.259***	-0.259***	-0.259***	-0.259***
p-value	(0.002)	(0.003)	(0.005)	(0.007)

Table A10: Heteroskedasticity and Autocorrelation Consistent Standard Errors: Effective Institutional Price

	Lag (0)	Lag (1)	Lag (2)	Lag (3)
Lagged Returns	-0.254***	-0.254***	-0.254***	-0.254***
p-value	(0.000)	(0.000)	(0.000)	(0.000)
Lagged Returns X Conduct	0.215**	0.215**	0.215**	0.215**
p-value	(0.017)	(0.023)	(0.029)	(0.033)

Table A11: Clustering: Institutional Aid

		Cluster	Cluster
	Robust	(Inst.)	(Inst. and Year)
Lagged Returns	0.215***	0.215***	0.215***
p-value	(0.000)	(0.003)	(0.000)
Lagged Returns X Conduct	-0.259***	-0.259**	-0.259**
p-value	(0.002)	(0.038)	(0.022)

Table A12: Clustering: Effective Institutional Price

		Cluster	Cluster
	Robust	(Inst.)	(Inst. and Year)
Lagged Returns	-0.254***	-0.254***	-0.254***
p-value	(0.000)	(0.000)	(0.000)
Lagged Returns X Conduct	0.215**	0.215*	0.215**
p-value	(0.017)	(0.073)	(0.038)

Table A13: Heteroskedasticity Consistent Standard Errors

		Log Lagged Endow	Lagged Endow
	Log Lagged Endow	Per FTE	Per FTE
Lagged Excess Returns	0.178***	0.222***	734,008***
	(0.044)	(0.050)	(59,482)

Table A14: Heteroskedasticity and Autocorrelation Consistent Standard Errors

		Log Lagged Endow	Lagged Endow
	Log Lagged Endow	Per FTE	Per FTE
Lagged Excess Returns	0.178***	0.222***	734,008***
	(0.066)	(0.073)	(89,715)

Table A15: Heteroskedasticity Consistent Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.222***	0.174***	-0.044	-0.011	-0.039
	(0.050)	(0.030)	(0.055)	(0.083)	(0.066)

Table A16: Heteroskedasticity and Autocorrelation Consistent Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.222***	0.174***	-0.044	-0.011	-0.039
	(0.073)	(0.038)	(0.069)	(0.093)	(0.077)

Table A17: Clustered Standard Errors

			Institutional		Effective
	Endowment	Spending	Grant Aid	Net Price	Inst. Price
Lagged Excess Returns	0.222*	0.174***	-0.044	-0.011	-0.039
	(0.113)	(0.053)	(0.101)	(0.124)	(0.097)